Bulletin of Concerned Asian Scholars

Vol. 6 , No. 2

April-August 1974

Committee of Concerned Asian Scholars

Statement of Purpose

We first came together in opposition to the brutal aggression of the United States in Vietnam and to the complicity or silence of our profession with regard to that policy. Those in the field of Asian studies bear responsibility for the consequences of *their research and the political posture of their* profession. We are concerned about the present unwillingness of specialists to speak out against the implications of an Asian policy committed to ensuring American domination of much of Asia. We *reject the legitimacy of this aim, and attempt to* change this policy. We recognize that the present structure of the profession has often perverted scholarship and alienated many people in the field.

The CCAS seeks to develop a humane and knowledgeable understanding of Asian societies and their efforts to maintain cultural integrity and to confront such problems as poverty, oppression, and imperialism. We realize that to be students of other peoples, we must first understand our relations to them.

The CCAS wishes to create alternatives to the prevailing trends in scholarship on Asia which too often spring from a parochial cultural perspective and serve selfish interests and expansionism Our *organization is designed to function as a catalyst*, a communications network for both Asian and Western scholars, a provider of central resources for local chapters, and a community for the development of anti-imperialist research.

[passed March 28-30, 1969, Boston]

Membership in CCAS is contingent upon acceptance of the Statement of Purpose. Yearly CCAS dues are $5. Donations above $5 are accepted from those who can pay more. This covers CCAS membership and a subscription to the Newsletter. Those who disagree with the Statement of Purpose, but would like to get the Newsletter may pay a non-membership fee of $6.

CCAS is divided into 3 regions each headed by a coordinator.

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Questions which cannot be answered within local chapters and/or relate specifically to the national organization should *be addressed to the regional coordinator in your area.*

All members are encouraged to relate to CCAS through local chapter activities so that members will have more positive and concrete political experiences, so that CCAS as a whole will remain responsive to members at the local level where political work is most vital, and so that members become increasingly aware of the link between events in Asia and situations within America. (If there is no local chapter near you and you are interested in starting one, write the regional coordinator for information.)

There are national CCAS projects which are in no way exclusive of local support and initiative. An Indochina Committee, So. Asia Committee, GI Support Committee, Philippines Committee and China Exchange Committee are nationally coordinated, but depend on local chapters for information gathering, analysis, and education on the community level where such activities are most effective.

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(front cover: "The People's Own Army"--popular print, 1930s, in the Chinese Civil War.)

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Chinese Foreign Policy: Toward Authentic Social Science

by Robert Boardman

The Committee of Concerned Asian Scholars has been described, in this *Bulletin*, as constituting "... a direct challenge to the intellectual and organizational hegemony of a whole group of scholars who, in spite of differences in values and personality, had a discernibly similar approach to scholarship. 'liberal' scholarship on Asia, most notably the vast outpouring from Harvard University which has defined Asian studies since the 1940s."[1] The purpose of this paper is twofold. 1) to point out that this body of scholarship, to the degree that it relates to Chinese foreign policy, has derived no significant inspiration from social science; and 2) to assert that the study of Chinese foreign policy would reap substantial rewards from the application of social science concepts and methods.

We would argue that there are in fact *two* direct challenges to established scholarship. The one we might term the radical critique. This addresses itself in essence to ethical considerations: a major theme is seen to be the moral status of a body of literature which seems to provide packaged rationales for government policies. From this standpoint, the analyst is drawn in to launch a thorough assault on the ideas, methods, and organization that nourished this literature. There is an alternative tack, however: the critique of authentic social science. Here, the guts of the attack are methodological. What is at stake is the *method* by which received wisdom on Chinese foreign policy is arrived at.

Towards *authentic* social science? The term is, or should be, redundant. But it serves to underline an important point. To repeat: the study of Chinese foreign policy has remained almost completely untouched by the procedures of the social sciences. To the degree that the radical critique fails to recognize this point, its thesis is weakened. This is unfortunate, as the procedures of social science proper are far from immune to just criticism. The radical critique, in other words, would be strengthened immeasurably if it were able to deal effectively not only with established scholarship, but also with the social science critique of established scholarship. This second element is conspicuously absent from the critical literature of the last few years.

This paper, then, is looking forward to a tradition of scholarship in Chinese foreign policy in which the analyst is, to use James Rosenau's telling analogy, playing the scientific game. Like any other scholar, he is trying to piece together a coherent and insightful account of his data. But "... unlike a player in any other game, he must do so in a way that renders his findings independent of himself. The distinguishing feature of the scientist's 'truths' is that they must be susceptible to confirmation by independent observers. Thus the rules of his game require him to make explicit the assumptions, concepts, and procedures that he used when he made his selections among all the phenomena in the international universe."[2] This game is played authentically if research questions are generated solely out of the interplay between the observer, his concepts and methods, his data, and his colleagues; if no extra-academic criteria intrude on this process; and if no nexus, whether informal and ideational, or formal and institutional, exists between the scholar and the policy community. On this set of criteria, the study of Chinese foreign policy has failed disastrously to become a responsible subfield of social science.

In particular, established scholarship has not provided itself with any mechanism for testing its most basic assumptions. Propositions are not phrased in such a way that they are capable of being disconfirmed. Instead, certain common themes, reiterated or implied *ad nauseam,* have permeated the studies of the post-1950 period like incense in a Byzantine cathedral. Some are historical, hangovers from nineteenth-century encounters with China. Others are more contemporary, products of the uncritical application to the People's Republic of China of models held earlier to be relevant in the Soviet case. At least twelve such themes have staked out the straight and narrow path through the minefields of critical thought:

i. Exotica sinica.

China scholars did not, of course, leave themselves so vulnerable to attack as to perpetuate older myths, though some may still, as Victor Purcell argued, have contrasted two images of China: the modern one of the totalitarian state, versus the more appealing Shangri-la of ceramics and landscapes, of cicadas and plum blossom.[3] But if China was no longer exotic, it remained at least unique: a special case of the communist world, prone to flights of irrationalism and unpredictability.[4] Hence the connection with American defense policy: if future Chinese behavior was uncertain, then containment policies (or, later, contingency planning against the worst case of threats to the stability of the Asian subsystem) had to be the *sine qua non* of twentieth-century encounters.

ii. Drive versus Response

Moreover, such policies, it was felt, could have no

fundamental effect on Chinese perceptions and objectives. Chinese policies in the world were products entirely of domestic factors: on a crude analogy with models in psychology, behavior was determined by in-built drives, rather than being responses to various stimuli.[5] Peking might complain of "encirclement," but such propaganda only demonstrated that policies were having the desired bite: Chinese drives were being held in check. China was being deterred from launching foreign adventures to support wars of national liberation, to stir up trouble for pro-Western regimes, or to divert attention from the kinds of domestic disasters held to be inevitable in such a state.

iii. Aggression

The assumption of aggressive intent was often so deeply ingrained as to rarely require explicit articulation in the academic literature.[6] It remained, nonetheless, the unavoidable first subject in the China-watcher's symphonic structures, to be re-stated, augmented, modulated, and harmonized, thus ensuring a thematic unity for each opus. To the suggestion that Chinese foreign policy had in fact been pragmatic and cautious, there was a reliable counter in the faithful second subject: that Chinese aggression had been effectively deterred by policies of strength from the Western side.[7]

iv. Personality

Aggressiveness, however, was not held to be a characteristic trait of the Chinese. It was, rather, a product of the leadership group. Hence the paramount importance attached to understanding the personalities of the men at the top. Biographical studies bulk large in the academic literature on Chinese foreign policy.[8] Great care was naturally taken to ensure that as little contact as possible was made with social science. Not even the most elementary of distinctions – between idiosyncratic and role variables – was allowed to impede the steady drizzle of facts about Mao's experiences in the civil war, Lin's personal ambitions, Chou's secret pro-Western leanings, this man's predilection for Russian tea, that man's peculiar relationships with his second cousin.[9] How much would it matter which individual happened to hold which office?

v. Totalitarianism

Totalitarian organization of the Chinese state and society gets us to the core of the image. China was held to be potentially aggressive because of the domestically insecure position of the regime.[10] Presumably discussions in the Politburo or Foreign Ministry went something like this.

> Comrade A: "I see our grain production figures are down this year. Pity about those floods"
>
> Comrade B: "It certainly is. Mr. Chairman, I move we have a war with India."

Or, to put it another way, external conflict behavior was held to be causally dependent on domestic strife in China: a hypothesis that went untested, and untestable perhaps, in the absence of social scientific procedures of enquiry.

vi. Foreign enemies

The idea of totalitarian regimes carefully choosing real or imaginary foreign enemies for domestic purposes recurs again and again in the literature on totalitarianism. It was soon adopted as an article of faith by many Chinese foreign policy scholars.[11] In the 1950s, the myth of American encirclement was blown up by Mao to mobilize the Chinese people; in the 1960s, American determination having paid off, Peking did a quick re-think and chose the Soviet Union to fulfill the same function. The possibility of threats to Chinese security being perceived by Chinese decision-makers was not considered.

vii. Propaganda and ideology

Models of foreign policy processes suggested in the literature of the 1950s and 1960s – for example by Snyder, Bruck, Sapin, Deutsch, North, Brody, Burton and others – often began with core assumptions about the importance of the ways in which foreign policy decision-makers themselves view the world. No such attempts were apparent in the literature on China, apart from simplistic observations about Mao "seeing" the United States as an obstacle to the achievement of his ambitions. There was some ambivalence about ideology: whether it should be interpreted as part of the process of post-decisional rationalization in China, or as part of the lenses that structured the distortions and irrationality evident in Chinese behavior. But either way, the assumption of aggressiveness was preserved intact.

viii. Strategy

Even where Sino-American relations were not viewed as a zero-sum game, each Chinese move was seen as "... a carefully plotted, conspiratorially conceived, predetermined plan whose every consequence is anticipated, every move has a secret meaning which detailed charting of protocol and word counting can uncover."[12] Thes quote is from Daniel Bell's critique of the literature of the Soviet Union, but it is just as pertinent in the Chinese case.

ix. International relations

This kind of simple model was extended to the whole field of China's relations with other states. In the 1950s China's relations with the U.S.S.R. were perceived by scholars as binding, hierarchical, and unbreakable.[13] Evidence of inner dissensions in the alliance was dismissed (often, interestingly, as "wishful thinking") as transient. Prevailing analytical models, in other words, were incapable of accommmodating the possibility of flux in the relations between states. a severe handicap to the work of any foreign policy scholar.

x. Irrationality

The most fascinating conflict between the dual images of Chinese leaders as either master strategists or hopeless paranoiacs emerged on the question of nuclear weapons. Was Mao mad to think he could "win" a nuclear war?[14] Or was he cunningly projecting an image of irrationality in order to mask the fact of Chinese nuclear inferiority?[15]

xi. Continuity and history

History, notoriously, can prove anything. In the case of China, the lesson of history looked ominous: fears of future aggression could be reinforced by references to historical expansionism and classical Chinese definitions of China's role in Asia. But the model lost nothing even if this road were not taken.[16] If the Peking regime was fundamentally discontinuous with anything previous in Chinese history, then the same fears could be supported from other sources: totalitarian organization, links with the Soviets, Marxist-Leninist doctrine, and so

on.

xii. Change and China's future

A final conflict between scholars related to the future. Was China likely to undergo a "mellowing," as the Soviet Union did some time after its Cuban adventure, when it finally learned that aggression was too costly?[17] Or was China "changeless," or at least likely to change its foreign policy goals only with a radical transformation of the state?[18]

Let us now consider three different kinds of responses to all this, corresponding to the three basic stands possible for a Chinese foreign policy scholar.

The response from scholarship in the traditional post-1950 pattern might be that all this is far too simple. We may, it might be argued, have thought along these lines in the 1950s, but not now: lurking in the woodwork somewhere there *might* be the occasional individual scholar who still takes a hard-line approach, but for most the heady days of Dullesian dogma are long gone. After all, most responsible Chinese foreign policy scholars have for many years supported policies of accomodation with Peking, and even U.S. diplomatic recognition. And this kind of criticism, moreover, takes no account of the changes in basic thinking that have taken place during the two Nixon administrations.

Secondly, the radical critique: One response here would be ethical: if such themes were held in common by both scholars and policy-makers, and if the actions of the latter, particularly in Southeast Asia, were morally reprehensible, then clearly the activities of the former are called into question. Another response might be to trace through these linkages more systematically: *why* was it that China scholarship in roughly the post-1950 period seemed to be so intimately tied to the day-to-day concerns of the policy community? What criteria determined the research policies of the major university centers, foundations, and non-profit organizations? How can we re-orient Chinese foreign policy studies so that a distinct cleavage is made with the record of the past?

But still another response of the radical scholar – and here we come to the nub of the radical-social science debate – would be substantive. The many assumptions implicit in traditional scholarship can be articulated and invalidated. Is it true that China is aggressive? No, China has a cautious foreign policy; and its actions are understandable when viewed in the context of American, and increasingly Soviet, policies in Eastern Asia. Are Chinese leaders irrational? No, this is merely an official American perception or rationalization: on some questions Chinese officials may view things very clearly and adopt highly "rational" policies. Is China totalitarian? No: obviously the system of government and the way the economy and society are organized are different from forms common in the West, but this does not mean that the Chinese people are suffering under duress the arbitrary rule of a small leadership faction. And so on.

This brings us to the third response: that of authentic social science. The social scientist cannot respond point by point in this way to the substantive assumptions common in established scholarship. His attack is, rather, *methodological.* As an area specialist with some intuitive knowledge of his field, he may *suspect* that the assumptions of the 1950s and 1960s are erroneous: he may even feel that he knows they are wrong. But his critique takes the form of a desire to reconstitute the bases from which knowledge is generated. He may still, as an individual, want to press for changes in attitudes to China. He may even want to press for certain kinds of initiatives from the policy community. But as a scholar his task is very much more limited. It will be grounded in the premise that ". . . as we continue to press for the policy changes that we suspect may improve man's chances for survival and dignity, we nevertheless continue that research which will permit us *to replace mere suspicion with relatively hard knowledge.*"[19]

Hence his attack on traditional scholarship: too often suspicion has been disguised as tested theory. And hence also his attack on the radical critique: too often from that direction different kinds of suspicions have enlivened the search for knowledge – often insightful, imaginative, thoughtful, provocative and coherent, but suspicions nonetheless. And, crucially, the radical critique contains within itself no mechanism whereby such hunches can be converted persuasively into knowledge.

The second argument of this paper is that the study of Chinese foreign policy would reap substantial rewards from the application of social science concepts and methods. What would such a field look like? To begin with, the social scientist is appreciative of the fact that he cannot simply "study China." He is aware of the dangers of selectivity: of, consciously or otherwise, seeking out the kinds of facts that will buttress a case, and the kinds of telling arguments that will make them crystallize, trying to contribute *both* to knowledge about Chinese foreign policy *and* to the general store of insights available to political scientists working in *any* subfield; from Malagasy local government to U.S. Congressional committees, from Indian interstate relations to Japanese energy policy. Without this sharing, a radicalized field of Chinese foreign policy scholarship would remain as isolated from the mainstream of ideas as its totalitarian predecessor: superficially satisfying perhaps, but ingrown; full of fury no doubt, but signifying little and noticed even less.

Some point of entry, then, is necessary on the theory-hypothesis-data circuit. The student of Chinese foreign policy could, for example, begin with any of a larger number of concentrations of theory within political science and straddling other social sciences. A conference held in mid-1972 under the auspices of the Center for Foreign Policy Studies of Dalhousie University brought together scholars working on such diverse threads as field theory, as used in social psychology by Lewin, as applied to international relations by Wright and Rummel, and as it could be applied to the study of Chinese foreign policy; alliance theory and its uses; the literature in sociology and political science on organizational behavior, bureaucratic decision-making, and interest groups, and studies of perception and cognition.[20] Clearly this list is by no means exhaustive. At the highest level of generality, articles on international relations of direct interest to the Chinese foreign policy scholar have been published in the *General Systems Yearbook,* with companion pieces on systems theory and parallel applications in biology, engineering, economics, meteorology, and other fields.

Working upwards from the data is another possibility. The analysis of data on "events" – discrete actions in interstate relations that can be coded and collected by the researcher – has become a flourishing enterprise in inter-

national studies in the last few years. Established scholarship can be criticized by the social scientist not only for failing to respond to theoretical developments, but also for failing to lay the groundwork for the accumulation of relevant data: facts, in brief, that can be stored and used by other scholars in the field for quick and efficient testing of hypotheses that interest him. Tools such as factor analysis may therefore by important for some Chinese foreign policy scholars. "Already the role that factor analysis plays in research is great. Much of the knowledge on personality, for example, is already factor analytic, and factor analysis has added considerably to our understanding of juvenile delinquency, crime, urbanism, primitive societies, national characteristics, conflicts, attitudes, and voting behavior, to name only a few topics."[21] When will we be able to say that much of the knowledge on Chinese foreign policy is factor analytic? Or when will China scholars be able to *reject* using such tools on the basis of a sound knowledge of what they are?

In short, we are arguing that the field of Chinese foreign policy studies should wrench itself away from the mystique which enveloped it in the 1950s and 1960s. We are arguing that scholars should be both area experts *and* social scientists; and that, in this latter capacity, they should be able to draw insights from middle-level theory throughout the social sciences, to develop a rich body of concepts relevant to the study of Chinese foreign policy, to draw up imaginative and perceptive hypotheses, and to facilitate the collection of pertinent data.

This is already beginning to happen. As the process continues, three important kinds of benefits will accrue:

i. Totalitarian mythology. It has been suggested here that the objection of the social scientist to the assumptions of established scholarship is methodological rather than substantive. So it is. But one consequence of a greater social scientific emphasis would probably be the death — perhaps we should say withering away — of the lumberingly top-heavy artifice of totalitarian mythology. Too many of its hidden assumptions about Chinese behavior just could not stand up to a rigorous exposure to more scientific procedures of enquiry.

ii. Knowledge not opinion. This assault, moreover, will enjoy greater persuasiveness than what we have here termed the radical critique. This suffers, in the last resort, by its failure to delineate a methodology by which one observer's judgments could be confirmed or disconfirmed by another *with different personal biases*. We are asked to rely solely on the individual scholar. There is no cumulative building of knowledge through successive enquiries in the same area. Social science provides, or should provide, researchers with some criteria outside of their own hunches and intuitions by which their work can be evaluated. The search is necessarily inter-disciplinary, but that does not mean that the social scientist is engaged in ". . . know-it-all eclecticism [which] succeeds in doing nothing more than avoiding the crucial issues of intellectual debate."[22] Neither the totalitarian paradigm nor its radical critique have even begun to come to terms with the crucial issue posed by authentic social science: the issue of knowledge, of what constitutes knowledge, of what distinguishes knowledge from opinion, and of the ways in which knowledge can best be arrived at.

iii. Effectiveness. The social scientist may be dismissed by the right for his subversive tendencies, while at the same time being derided by the left for his boot-licking subservience to "liberal" norms. The man in the middle, it would seem, is a man in a muddle. But if the social scientist is instrumental in his research in destroying the cornerstones of the totalitarian paradigm of Chinese foreign policy studies, and if he does so in a manner that is self-evidently not idiosyncratic, then surely his argument would be that much more effective. The danger of a non-scientific critique of established scholarship is that it can be dismissed glibly because it can be neatly labelled. The policy community, and many China scholars themselves, remain untouched: "irresponsible" critics and "cranks" are easy game. This glib dismissal is doubly unfortunate. It means that the important questions raised, for example, in this *Bulletin* are given, at best, scant attention. It means also that an atmosphere of professional self-questioning may continue to remain alien to the field.

Social science enjoyed a bad press in the United States in the 1960s. The experiences of that decade seemed to demonstrate that social scientists were little better than well-paid camp-followers. Too often academics gave the appearance of being involved, willy-nilly, in the dubious process of rationalizing the irrational, of giving detailed map bearings for the lurching grind of the policy tank. Social scientific procedures, however, can be used intelligently, or they can be abused. The manufacturer is not necessarily to blame if the car crashes while its driver is drunk. This paper, then, is a plea for their intelligent use. We now face a situation, in the mid-1970s, of mutual suspicion between members of the three primary communities engaged in research on Chinese foreign policy: traditional scholarship; radical scholarship; and authentic social science scholarship. In the case of the last two, the suspicion is grounded in an extraordinary degree of mutual ignorance, some of which this paper has tried to dispel.

Notes

1 *Bulletin of Concerned Asian Scholars*, Special section: "Imperialism in China," editor's note.

2. James N. Rosenau, "Games International Relations Scholars Play," in his *The Scientific Study of Foreign Policy* (New York: Free Press, 1971), 16.

3 Osbert Sitwell, *Escape with Me! An Oriental Sketchbook* (London: Macmillan, 1949), Arthur H. Smith, *Chinese Characteristics* (New York: Fleming H. Revell, 1894). For other accounts, see K. S. Latourette, *The Chinese: Their History and Culture* (New York: Macmillan, 1942), and Chester Holcombe, *The Real Chinaman* (New York: Dodd, Mead, 1895). Western responses are discussed in more detail in H. R. Isaacs, *Scratches on our Minds. American Images of China and India* (New York: John Day, 1958); R. Iyer, ed., *The Glass Curtain between Asia and Europe* (London: Oxford University Press, 1965); Raymond Dawson, *The Chinese Chameleon: An Analysis of European Conceptions of Chinese Civilisation* (London: Oxford University Press, 1967); John M. Steadman, *The Myth of Asia* (New York: Simon and Schuster, 1968).

4. Arthur S. Y. Chen, comment in Richard F. Staar, ed., *Aspects of Modern Communism* (Columbia: University of South Carolina Press, 1968), 224. On the question of unpredictability, see further Z. K. Brzezinski, *The Soviet Bloc. Unity and Conflict* (New York: Praeger, 1960), 393-94.

5 Benjamin I. Schwartz, "Modernization and the Maoist Vision: Some Reflections on Chinese Communist Goals," republished in his *Communism and China. Ideology in Flux* (Cambridge, Mass: Harvard University Press, 1968), 163. The same model has been sketched in by Halperin and Perkins: ". . . the main foreign policy goals

of the Chinese Communist leadership are highly stable and, therefore, will probably have potent and continuing influence on action. . . . Foreign policy goals, however, are not the only relevant or predictable determinants of China's foreign policy actions. Some of these relevant and predictable elements are found in the domestic economic and political resources of the regime. China's economic might, for example, is severely limited, and this is turn inhibits Peking's ability to exercise influence over other nations through aid and trade." (Morton H. Halperin and Dwight H. Perkins, *Communist China and Arms Control* [New York: Praeger, 1966], 1.) See also Robert Strausz-Hupé, W. R. Kintner, and S. T. Possony, *A Forward Strategy for America* (New York: Harper, 1961), 37.

6. Kurt London, *The Permanent Crisis* (Waltham, Mass.: Blaisdell, 1968), 151; cited by Robert C. North, *The Foreign Relations of China* (Belmont, Calif.: Dickenson, 1969), 2; Samuel B. Griffith, *The Chinese People's Liberation Army* (New York: McGraw-Hill, 1967), 8; O. Edmund Clubb, "The International Position of Communist China," *The Fifth Hammarskjoeld Forum*, ed. L. M. Tondel (New York: Oceana, 1965), 35; Edwin O. Reischauer, *Beyond Vietnam: The United States and Asia* (New York: Knopf, 1968), 163, cf. George E. Taylor, "The Foreign Policy of Communist China," in Roger Swearingen, ed., *Soviet and Chinese Communist Power in the World Today* (New York: Basic Books, 1966), 56-65.

7. Melvin Gurtov, *Problems and Prospects of United States Policy in Southeast Asia* (RAND RM-5910-ISA, March 1969), 14, 24, 43; Roger Hilsman *To Move a Nation. The Politics of Foreign Policy in the Administration of J. F. Kennedy* (New York: Doubleday Anchor, 1967), 326, 346, 349; cited by Ross Terrill, "John Carter Vincent and the American 'Loss' of China," in Bruce Douglas and Ross Terrill, eds., *China and Ourselves. Explorations and Revisions by a New Generation* (Boston: Beacon Press, 1970), 145.

8. Harold C. Hinton, *China's Turbulent Quest: An Analysis of China's Foreign Relations since 1945* (London: Collier-Macmillan, 1970), 289.

9. Sinologists also "spend much of their time trampling over flower beds in order to point out familiar trees": Martin Bernal, *New York Review of Books* (March 23, 1972), 31-32. And:

> *Interpreting history by analyzing the careers of leading figures is a delightfully facile method, [one writer admits, but then hastily adds an argument for the centrality of trivia, since in China] a preference for green neckties, or none at all, may be quite significant.*

Robert Elegant, *China's Red Leaders* (London: Bodley Head, 1952), 246.

10. C. J. Friedrich and Z. Brzezinski, *Totalitarian Dictatorship and Autocracy* (Cambridge, 1956), 9-10. A. Doak Barnett, "Group Indoctrination," in Stewart E. Fraser, ed., *Education in Communist China: An Anthology of Commentary and Documents* (London: Pall Mall, 1971), 281. See also Theodore H. E. Chen, *Thought Reform of Chinese Intellectuals* (Hong Kong: Hong Kong University Press, 1960); Robert J. Lifton, *Thought Reform and the Psychology of Totalism: A Study of "Brainwashing" in China* (New York: Norton, 1961). See also Alexander Dallin and George W. Breslauer, *Political Terror in Communist Systems* (Stanford: Stanford University Press, 1970).

11. Hannah Arendt, *The Origins of Totalitarianism* (Cleveland: Meridian Books, 1958). Harold Hinton, *Communist China in World Politics* (Boston: Houghton Mifflin, 1966), 492. Hinton also appears to be attracted to the theory that the Chinese have adopted Voltaire's comments on God. The CCP, he writes, "tries to maintain a seemingly wide, although spurious, domestic consensus partly by projecting as much hostility as possible outward against the most conveniently available target, which since 1945 has happened to be the United States."

12. Daniel Bell, "Ten Theories in Search of Reality: The Prediction of Soviet Behavior," *World Politics*, 10 (April 1958), 327.

13. Stuart Kirby, "Russia's Largest Satellite," *China Quarterly*, 1 (January-March 1960), 13.

14. Richard L. Walker, "The Elusive Elan: Problems of Political Control in Communist China," in Richard F. Staar, ed., *Aspects of Modern Communism* (Columbia: University of South Carolina Press, 1968), 206.

15. See G. H. Quester, "On the identification of real and pretended Communist military doctrine," *Journal of Conflict Resolution*, 10 (1966), 172-79; and further on this point, Robert Boardman, "China's Rise as a Nuclear Power," *Yearbook of World Affairs* 25 (1971), 63-64.

16. History, and especially Chinese history, as Fairbank has pointed out, can be selected on all sides of a question. See J. K. Fairbank, testimony in *United States Policy with Respect to Mainland China, Hearings.* Committee on Foreign Relations, United States Senate, 89th Cong., 2d. Sess. (Washington: USGPO, 1966), 98.

17. H. Gordon Skilling, "Interest Groups and Communist Politics, *World Politics*, 18:3 (April 1966), 436-7.

18. For negative views: George E. Taylor, "The Foreign Policy of Communist China," in Roger Swearingen, ed., *Soviet and Chinese Communist Power in the World Today* (New York: Basic Books, 1966), 73.

For positive views: Hinton, testimony in *United States Policy with Respect to Mainland China*, 380; and Walter C. Clemens, *The Arms Race and Sino-Soviet Relations* (London: Collier-Macmillan, 1970), 293.

19. J. D. Singer, Stuart Bremer, and John Stuckey, "Capability Distribution, Uncertainty, and Major Power War, 1820-1965," in Bruce Russett, ed., *Peace, War, and Numbers* (Beverley Hills, Ca.: Sage, 1972), 48. Emphasis added.

20. R. L. Dial, ed., *Advancing and Contending Approaches to the Analysis of Chinese Foreign Relations* (Halifax, Nova Scotia: Dalhousie Univ. Center for Foreign Policy Studies, 1974).

21. R. J. Rummel, *Applied Factor Analysis* (Evanston: Northwestern Univ. Press, 1970), 5.

22. *Bulletin*, Special section, 2.

A Bibliography on Women in China

For the past year and a half a group of people in anthropology, sociology, history and geography at UCLA have worked continuously on a comprehensive bibliography concerning *Women in China.* The bibliography now includes approximately 6,000 titles, both primary and secondary materials, classified by time period, geographic location and subject, and stored on computer tapes for easy retrieval.

In our attempt to draw together all the English-language published sources we have included materials on women in the People's Republic of China, Taiwan, Hong Kong, and general works about Chinese women without a particular geographical reference. No subject nor time period was excluded, although our compilation of sources became more and more comprehensive as we approached the present.

The mimeographed bibliography may be obtained in two forms: (1) an alphabetical listing by author or title for English-language works, and by file number for the translated articles. (2) a listing by topic or subject: biography, general works, literature and art, labor and production, education, health and welfare, marriage and the family, politics and government, and other. An entry may appear more than once.

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Chinese Foreign Policy and American Social Science

by Edward Friedman

Karl Marx and Mao Tse-tung, C. Wright Mills, Barrington Moore, Jr. and Fei Hsiao-t'ung also described themselves as social scientists. Thus when Boardman contrasts radical scholarship with authentic social science he is merely using a debater's tactic, using a label to place his adversaries at a rhetorical disadvantage, trying to monopolize the term science for only his outlook on events.

In reality there are many and different kinds of social science. Some stress conflict, others consensus. Some begin with a question, others with a theory. Some begin with material reality, interests and the like, others with ideational factors, values, etc. Some identify with the potential project of freedom and reason, others rationalize what is in terms of the social givens. While some focus on dynamic subject-object (or subject-subject) interactions, even insisting that knowledge is inherently ternary, others argue that the external object itself can be known by a separate observor. Some (e.g., Marx, Weber) comprehend on-going reality by theories specific to types of historical societies; others (e.g., Parsons and Skinner) prefer trans-historical theories of a universalistic character. The number of debatable positions and combinations of a serious kind could be effortlessly extended.

Some of these social science perspectives make great sense to me. Others do not. If I identify with a Sartre-Mills-Moore humanist phenomenology of social sciences[1] — or Piaget[2], or R. D. Laing, or Lévi-Strauss, or Szasz, or Chomsky — then in each case I have chosen against other forms of social science. Either Boardman wishes to fool us by posing as a spokesman for *the* social sciences or he is unwilling or unable to discuss the real choices we are all compelled to make.

Actually Boardman seems to identify with one variety of social science — trans-cultural, universalistic, sharp subject-object distinction, etc. — and pretend that it is the only and authentic social science. His labelling it as such does no more to make it authentic than does a Taipei restaurant being named Chen Pei-p'ing.

Why should we adopt Boardman's personal preferences? Supposedly because its methodology guarantees genuine, hard[3] and persuasive knowledge. But Boardman informs us that this methodology only produced error and apologies for murderous policies in the 1950s and 1960s. This is supposed to be because the methodologies were not used intelligently. And this abuse of good methodologies is supposed to stem from too close affiliation with ruling groups. Therefore, according to Boardman, more independence is needed. Boardman offers not a scintilla of evidence of any move toward such independence. Leading social scientists cum area specialists who have been utilizing these expensive techniques have all, to the best of my knowledge, been related to the U.S. Government: Robert North (U.S. Navy), Richard Solomon (National Security Council), Allen S. Whiting (Department of State). I do not intend this listing as support for Boardman's vulgar Marxist notion that knowledge or perspective stems only from work interest. The people listed are quite diverse. I merely wish to stress that to adopt Boardman's methodology, one must have faith in a method which he accords no proven results and for which he offers no evidence of change from the cause to which *he* attributes (on the basis of authentic social science or a mere suspicion?) the bad results. Thus one can believe in Boardman's personal variety of social science only as a matter of ultimate faith, a truly religious proposition where one is supposed to believe because it is absurd.

Nothing seems to have changed in the recent record. Even while Henry Kissinger was preparing to fly to Peking, leading mainstream social science students of Chinese foreign policy such as Michel Oksenberg and A. Doak Barnett were explaining why no basic change was possible in U.S.-China relations until Mao Tse-tung died, as if Mao were the obstacle to better relations.[3a] The CCAS *Bulletin*, meanwhile, published an article titled "The Nixon-Mao Pact." A few years earlier, Bruce Douglass and Ross Terrill edited a collection of essays called *China and Ourselves* containing an article arguing, against mainstream social scientists, that the Chinese position on Taiwan affairs was no obstacle to improved U.S.-China relations. There is nothing from mainstream social scientists to place in a scale to weight against the contributions of critical students of Chinese foreign policy, e.g. work such as I. F. Stone on Korea, Neville Maxwell on India, Stephen Fitzgerald on the overseas Chinese, John Gittings, David Mozingo and Gregory Clark on a host of other topics. One might think that the facts of the matter might bother Boardman except that his polemical intent seems to have led him to forget even the simplest of facts.[4]

Whereas Boardman thinks the lessons of Chinese history were used to prove the People's Republic expansionist, the literature is a bit more muddied, with leading historians such as John Fairbank and C. P. Fitzgerald using the data to argue against likely Chinese expansion against the American-led

camp. Actually inauthentic social scientists and traditionalists of the China-watcher and America-watcher variety in Peking and Washington have a better predictive record than any non-China watcher social science purchaser and purveyor of the expensive technologies that I know of.

Boardman has only contempt for government people and radicals. He caricatures both and understands neither. Thus he congratulates radicals for being right – if unpersuasive – about China pursuing a cautious foreign policy. But that China was cautious is the argument made in the U.S. Department of State, CIA and DOD by those arguing for bombing northern Vietnam as a low-risk policy, by MacArthur arguing that the Chinese would not enter Korea, by the Indian government (and DOD and CIA) pushing north of the border contending that China wouldn't dare respond. What critical scholarship contributed was an understanding of the high risks the Chinese faced, whether or not they acted with military decisiveness, because of the military power and aggression of the American side.

Boardman writes with the monopolistic (totalitarian?) religious passion of so much of mainstream social science, claiming that its method is the only method. He tries to humiliate his intellectual opponents. Traditionalists are mocked as primitives for focusing on ideology, "experiences," "ambition," "leanings," and "predilections" of members of ruling groups in China, whereas Boardman would investigate core assumptions about how decision-makers view the world. That Boardman cannot see that this is a distinction without a difference stems, I believe, from his total commitment to one school such that he cannot see that his adversaries on all sides are well-informed, able people with grounding in profound intellectual traditions.[5]

Boardman's manhandling and mishandling of the central concept of totalitarianism (which he ignorantly describes as cruel and arbitrary rule by a small leading faction) can reveal the trivializing consequences of a social science approach which can only identify with the latest fads of modernists. He argues that totalitarianism is an isolated ideational element outside the mainstream of ideas. To be sure, cold war social scientists used the concept of totalitarianism as a bludgeon to discredit the great human gains of the Chinese revolution, but this condemns those mainstream thinkers, not the concept. Properly used, the notion of totalitarianism can help identify genuine and great dangers to the advance of that revolutionary project.

Although it can be traced back to the times of Luther or Cromwell, the concept of totalitarianism has been central to Western social theory at least since the French revolution. The new universal nation-state seemed opposed to the abstracted social atom of the privatized individual while demanding a synthesis in proper public citizenship. Terror was seen as the result, vulnerable society exposed to the sovereign state. This opposition of liberated egos lacking mediating superego groups of protection and realization, forced instead to stand alone and yet identify with the all-powerful religion of the nation, confronts us all. The horror of totalitarianism is one potential of the era of industrializing and industrialized nation-states, of China as well as of America, Japan as well as Germany or the U.S.S.R. The elements which led Moore and Marcuse to develop the notion of popular totalitarianism to account for certain tendencies in the industrialized West are not confined to the West. In fact, the Cultural Revolution cannot be fully understood without theories about what made for the strength of those forces threatening a totalitarian catastrophe against which Mao tried to mobilize popular and other forces.

But Boardman wants social scientists to demolish the notion of a totalitarian China, as if the Nixon-Kissinger foreign policy weren't already achieving that goal as the global cold war of Harry Truman's time institutionalized that notion of a totalitarian China. With rapprochement come new categories. Boardman is at one with the new politics. If some anti-Nixon forces who wish to control Asia in league with Japan have their way, China can be returned to the category of demonic totalitarianism. All is marked and marred by politics. It is difficult to ever avoid being deeply influenced by the political definitions of ruling groups, but it may be well-nigh impossible without some serious grounding in these deeply rooted intellectual traditions.

Boardman cites only contemporary scholars without mentioning their intellectual roots as if these traditions of comprehension have nothing to teach. All critical scholars in social science departments face this intellectual barbarism which insists that the language of computers has more to teach than Hegel, de Tocqueville, Marx, Pareto, et al. Boardman embodies this barbarism when all he can do with the descriptions of Maoist politics as irrationality is dismiss it as "exotica sinica." Yet the notion of reason as explicated in a traditional discourse encompassing Hegelians and Weberians is central to our understanding of the human project. Our prospects and hopes can be related to a substantive notion of reason encompassing freedom and fulfillment or to a technical mean-ends cost-benefit calculus with no necessary bond to human joys. That the Maoist vision challenged the latter perspective, the Weberian notion of reason, in questioning the inevitability of the transition to impersonal, bureaucratic, urban, expert culture may have in the first instance led to a condemnation of that vision as impossiblist, inhuman and irrational. But surely that is no reason to dismiss the categories and the concerns. Indeed it is one of the successes of CCAS scholarship that much of the profession now treats such matters with the utmost seriousness.

Social science in both Stalinist and mainstream American guises has proved itself more ideology than critical comprehension. Events are not experienced. They are placed in pre-existing theoretical categories. Following this methodology one almost invariably finds the data that rulers demand. All American acts are expansionist. Nothing is learned. Boardman offers us a solution which is in fact a part of the problem. He cites a book of Yale's Bruce Russett to help us gain hard knowledge. Russett, in a January 1967 SEADAG paper, found only tenuous hope than non-communist Asia would perceive "revolutionary," "xenophobic" and "expansionist" China as the threatening common enemy that Russett believed it was. "Either the United States must indefinitely maintain a humanly and materially expensive presence in the Asian rimlands, or it must reconcile itself to living in a world, a tightly integrated system, where the Middle Kingdom has restored its control over Southeast Asia, and India and Japan must live in awe of it." In addition, Boardman cites James Rosenau on behalf of a true science of independent observers. Publishing in 1968 and 1969 Rosenau wrote, "Some linkages may in fact be founded on enmity and be highly disintegrative for polities, international systems or both. Communist China's linkages to its environment are a case in point." "The advent

of thermonuclear war and the emerging of Red China as a budding and recalcitrant superpower are illustrative of another historical trend that has fostered a reorientation in foreign policy analysis, namely, the emergence of problems that are worldwide in scope."

The people who according to Boardman possess the secret techniques to lead us to truth here parrot the ideology of ruling powers. International relations specialist A. F. K. Organski concludes the 1968 edition of his text on *World Politics*, finding that "China's recent provocative behavior ... increasing the chances that the challenger America will face will be China and not the U.S.S.R. ... [means] the chances of war between China and the United States are higher," a prediction Jean Dixon had already offered on New Year's Day 1962. Following changes in line and in lines such as these can serve as an index of the scientific worth of the literature. To be sure, these scientists have now adapted to the Nixon-Kissinger redefinition of China as just another major nation-state. But that is the point. Their methodologies lack any independent, critical perspective. One need not even offer evidence against them because in a new period their circular quality on behalf of ruling policies is immediately exposed. K. J. Holsti explaining "The Use of Objective Criteria for the Measurement of International Tension Level" (*Background*) in August 1963 is another instance of these subjective tautologies. He found:

> *The referrent for an index or scale of tension level is the degree of national and governmental commitment to the expansion of a political or social ideology. . On the ten point scale, for example, Communist China might receive a maximum value, the United States and the United Arab Republic would receive medium values ...*
> *Assigning values on an impressionistic basis pending the development of exact measuring techniques. ... Communist China would receive a high value because of the ideological rigidity of its key decision-makers and the element of secrecy surrounding the formulation of foreign policy tactics. Nazi Germany in 1944 would receive a very high score. ...*

These are all instances taken not from those whom Boardman finds naive students of China but from the wielders of sophisticated methodologies that produce, he says, not hunches but real knowledge. The evidence is not in harmony with Boardman's hypothesis. He further suggests for our consideration "a flourishing enterprise in international studies" combining the "accumulation of relevant data" and "theoretical development," namely, "the analysis of data on 'events' – discrete actions in interstate relations that can be coded and collected by the researcher. ..." The most expensive and praised mainstream social science study of China is of this category. It was produced by a team headed by Charles McClelland working for the U.S. Navy's Project Michelson and titled "The Chinese Communist Performance in Crisis and Non-Crisis: Quantitative Studies of the Taiwan Straits Confrontation 1950-1964."

What should be minimal standards for deciding that the result of the money and method was "sound knowledge"? It should at least identify the three main periods of crisis, 1954-5, 1958 and 1962. It doesn't. That 1962 crisis which has been written about by Hilsman, Sorenson and Schlesinger among others is completely missed. Instead of describing how pro-Chiang Kai-shek forces almost involved America in a war with China, McClelland described 1962 as a "non-crisis" period which was part of a larger "post-crisis period" when "nothing spectacular" happened. Reading the unsophisticated people at *Jen-min jih pao*, URI, and RAND in 1962 leads to a very different conclusion, but they didn't limit their scientific data-collecting to the *New York Times*.

Surely even a college freshwoman's knowledge of social psychology which introduces her to the results of the Rosenthal experiments[6] indicates how naive and unscientific it is to treat data as discrete and independent of the expectations of the participants. McClelland's attempt to give the true score of *the event in itself* as tension-increasing, tension-reducing or noise-making is therefore flawed from the outset. Thus when Chou En-lai on March 6, 1957, charged that America planned to install guided missiles in Taiwan, McClelland records it as an accusation, a noise-making act. Since McClelland's work presupposes a model of deterrence where American actions restore a status quo and thereby end a crisis, he simply cannot ask whether Peking's dominating in the accuse category might indicate that Peking was under the gun and that the major sources of conflict lay in the American system. Consequently America's September 15, 1958 announcement that it was sending missiles to Taiwan, the September 28 sailing of such missiles, and the October 18 announcement of the turning over of this weaponry to the Chiang Kai-shek government are recorded as acts of support for an ally which are tension-reducing acts. Yet the reader needs no argumentation to decide that Khrushchev's 1962 support of Castro with missiles was not quite tension-reducing for a Kennedy administration worried about seeming a pushover to Moscow and a softie to the American people. These supposedly sophisticated methodologists thus turn out to be the most simple-minded and, if we can take Boardman's word for it, "well-paid camp followers."

The naive ideological character of the literature under discussion is so obvious that is is puzzling why Boardman writes so positively of the "social scientists" who create these thoroughly misleading imitations of a scientific enterprise. R. J. Rummel's use of "factor analysis" is a case in point, and worth examining since Boardman approvingly cites this approach three separate times in his essay.

Publishing in 1966 in the *Journal of Peace Research*, Rummel presented the results of factor analysis applied to 94 variables on the foreign behavior of nations from 1955 to 1957. This was the period, most any student of Chinese foreign policy would agree, when China was kept out of international organizations by the United States, in which China was forced into combat against Chiang's U.S.-backed forces to end coastal blockade and armed attacks, in which the U.S. wanted to keep newsmen from China, to keep Japan and Europe from increasing significantly their trade with China, in which Chinese protested and marched against these insults and attacks and promised a day of reckoning. While a weak China was being victimized, a powerful America under the Dulles brothers was sending CIA personnel and military groups to Asia to subvert foreign governments and harass China, backed by the threat of massive retalliation should China try to challenge these aggressive ventures. This picture of China in the Bandung era of peaceful co-existence and America in the cold war era of Dulles appears nowhere in Rummel's computer study. How come?

Rummel concludes that there were two distinct and basic patterns of international behavior, participation linked to

a dimension of military expenditure and conflict which includes anti-foreign demonstrations, protests, countries with diplomatic relations severed, threats, accusations, etc. America's power to dominate and exclude is labelled (P) participation, where P = aE + bC, a and b being constants, E economic development and C power capability. China's victimization is called conflict. America is defined as "protecting the current officially sanctioned international configuration of power and rewards." *China is defined as "attacking it."* All is circular and definitional with the victim called the culprit. The symmetry with McClelland's approach is complete. There when China shoots down a U-2 invading its air space, that is recorded as an act of aggression, a tension-increasing act on China's part. And when Taiwan acknowledges on the next day that they sent the intruding plane up, that admission is considered a tension-reducing act. The victim is the aggressor.

Would Rummel's data have better predicted future involvement in conflict if his measures had been, for example, the value of military aid, military bases overseas, military personnel overseas, expenditures on foreign intelligence and subversion, number of military treaties, intelligence personnel overseas, percentage of military expenditure on major long-distance warships, bombers, troop transports and the like? Would America then have been excluded from the conflict pattern? Would not China have scored quite low on such a dimension? But could Rummel afford to choose variables leading to the conclusion that America was a major source of international conflict? The factors he chose – not the international world as such – produced the patterns he found. And since the data selected for both McClelland and Rummel stems from the assumption of a framework in which America polices the world to deter conflict, that data is rooted in particular frameworks such that no meaning can be given to the idea of running the data through the machine correctly. The theory already inheres in the choice of variables and data. Rummel was the victim of and his scholarship the embodiment of his ideology. Writing in terms of Western, neutralist and Soviet bloc countries, he actually defined as the ideological dimension of international behavior whether one stood against the American position at the UN. "Given the bipolar nature of much of mid-1950s international politics, the importance of the variable is clear. It is rewarding, however, to have what is 'self-evident' confirmed by systematic analysis." The "self-evident" actually was the rationale for power groups in the United States and Rummel merely provides the respectability of science for the practice of power. What does any of this work have to do with "sound knowledge"?

My point is not that there is nothing to learn from social science, whose traditions are far richer than Boardman's essay would have us believe. Rather I want merely to highlight the naivete of believing that some occult technique produces the truth, believing that we can escape the limits of our time and place, of believing that studies of contemporary international relations can be other than political and value-laden. The consequence of abandoning a value-based critical perspective for a spurious objectivity is, as with McClelland et al, that one becomes a spokesman of the dominant ideas of ruling groups.

Boardman's essay begins by identifying social science with genuine methodologies, but ends by finding the issue to be the "intelligent use" of those methodologies without spelling out what "intelligent use" means. However, if one can judge by Boardman's criticism of scholars of the 1950s for not stressing the inner tensions of Sino-Soviet relations, then correct prediction seems to be one index of intelligent use. Yet even a glance at Russett, Rosenau, Holsti and Organski above shows them far from prescient. On Sino-Soviet relations, is Boardman unaware that CIA analysts, as well as Allen Whiting and Benjamin Schwartz, did write insightfully about tensions between Moscow and Peking in the 1950s? The political question must be put. What political forces and fears kept them from going further with their evidence than they did? Here again critical scholars who have chosen to associate with CCAS have made valuable contributions. They have pursued inquiries about the dominant ideas in the profession and the social logic of that idea set; they have pursued questions of power and funding in the profession, relating this concern to foundation power-brokers and governmental needs; and they have placed these researches within the context of area studies in general, the cold war in general.

One conclusion of this work is that, contrary to Boardman, it is not so easy to be independent of "the policy community" since its tentacles – including its power to define reality itself – extend powerfully into the academic community and can invidiously label and penalize those who dissent and thus evidence their irrationality and irresponsibility. Taking into account the oppressive political context leads me to conclude that Whiting and Schwartz, rather than being condemned for methodological naiveté as Boardman would have it, might better be praised for courage or unusual acumen. Does Boardman believe that – given such events as Sheila Johnson's McCarthyite condemnation in 1972 in *Commentary* magazine of virtually all who return from China with praise for being fellow-traveling apologists for Stalinist totalitarianism, plus academic purges and the tightening of the job market – such matters are of no consequence in one's ability to fearlessly pursue truth? When Boardman warns against a radical critique "because it can be neatly labelled," he states the truth. Institutionalized McCarthyism is an integral part of social science.

Surely Boardman is aware that appeals for the application of social science to the study of China have appeared regularly.[7] Since scholarship should consciously comprehend its own traditions, I expected Boardman to relate to that literature, to build on it or dissent from it. Boardman claims social science is subversive. I missed that point in Chalmers Johnson's polemic on the topic. Had Boardman explained how Johnson et al. had misunderstood the issues I might better understand why another polemic is necessary. Is it to persuade the Ford Foundation to fund factor analysis of the fit of various theories of imperialism with variables of American and Japanese foreign policy?

I do not in the slightest intend this as a dismissal of the rich modes of social theory available to us. Indeed, I agree with Boardman that Kurt Lewin is a genuine part of the mainstream of ideas. But instead of merely dropping the name, might not Boardman point out that Lewin studied with Wolgang Kohler and tried to combine the insights of gestalt theory and structuralism? A word might then be added concerning structuralism in general mentioning, say, F. de Saussure, Chomsky and Lévi-strauss. But I am beginning to suspect that mainstream thinkers such as these are studied more by critical scholars of the CCAS variety than by the factor analysts whom Boardman praises. And if one were to enter further into these mainstream thinkers dealing with

phenomenology, existentialism, Marxism and critical theory, one would also be discussing Sartre (e.g. *Search for a Method*), Merleau-Ponty, Jürgen Habermas et al. Somehow I feel that this is not the mainstream that Boardman intended. Yet if Boardman were part of that mainstream of concerns, then instead of trying to pawn off long discredited verifiability criteria, he would work with the notion that both facts and theories are epistimologically mediated and treat seriously the growing body of work which comprehends social science as a commodity produced by certain types of historical societies, and, consequently, a kind of knowledge which might be transcended by societal changes.[8]

Instead Boardman roots his "desire to reconstitute the bases from which knowledge is generated" as a struggle of himself as a centrist against extremes of both left and right. Of course Mao Tse-tung presents his views in the same way. Me too. I stand between the apologists for the horrors committed by the powers that be and the infantile left so alienated by that disgraceful form of intellectuality that they have abandoned all attempts to link freedom and knowledge. As a centrist I avoid both extremes and maintain a true intellectual's commitment to joining freedom and knowledge in theory and practice. From the perspective of sociology of knowledge and philosophy of science one might ask why true knowledge seems related to balance, a query which could build on Thomas Kuhn's insights flowing from his study of the idea of balance and *The Copernican Revolution* and the thesis that presuppositions of accepted forms of social knowledge replicate the assumptional forms of the dominant theoretical mode for comprehending nature.

Thus if he wishes to present himself as a philosophically knowing apostle to the unknowing traditionalist pagans, Boardman should stop promoting the "theory-hypothesis-data circuit" as some universal road to truth and instead acknowledge the existence of a Kuhn-versus-Popper debate, paradigm versus hypothesis, puzzle solving versus problem solving, dogmatic parameters versus open dialogue. A bit more attention is required to Kuhn (and Feyerabend and the attempted synthesis by Richard Bernstein, *Praxis and Action*) as to why dominant social science methodologies – if properly defined by Kuhn – cannot, as the Popperians correctly point out, be a liberal middle road to truth.[9] That is, if it is the controllers of the successfully industrializing nation state that define the international relations paradigm, then it is no mystery why, when these ruling groups act so as to redefine the China paradigm from that of an especially dangerous nation to that of just another major nation state, there is no decline in professional prominence for those social scientists who had claimed that one could not deal with China until that irrational and romantic Mao Tse-tung died and no rise in professional standing for critical analysts who had long contended the opposite. Also, the role of establishment social science as exposed in all the instances I cited above is clarified: its role is not to pursue sound knowledge but loyally to solve puzzles within the paradigm established by ruling groups.[10]

I would like to conclude on a constructive note and establish once and for all Mao Tse-tung's just claim as the great authentic social scientist, a veritable Einstein of social theory. Empirical data from three disciplines can be drawn upon to independently establish this monumental fact. First, as any China-watcher knows, Mao Tse-tung thought is referred to as a spiritual atom bomb. As Kuhn's insights would lead us to suspect, Mao has tried to adapt his thinking to a Planck-Einstein conception of reality. Mao loves to ask physicists about such issues. *Red Flag* has written on the topic. And in 1963 Mao summed up the anti-Newtonian idea that matter was no longer to be conceived of as an essential, unsplitable basis of all things. Instead Mao offered what has been described as his "great theory," that "matter can be transformed into consciousness and consciousness into matter." Second, intellectual historians of China have discovered that Mao has described the possibilities for creativity and construction by the masses in terms of a metaphor with nuclear energy. Our "nation is like an atom. . . . When this atom's nucleus is smashed, the thermal energy released will have really tremendous power. We shall be able to do things which we could not do before. . . ." Clearly one must divide into two and into a great deal more as the chain reaction continues. And third and finally, a social scientist of formal organizations need not be satisfied by such vague and exciting ideas. He can evince the obvious superiority of Boardman's authentic social science methodologies and can give a precise theoretical relationship to the mass line, the political formula for this explosive reaction. Mao has explained that the ruling elite (E) are to relate to the cadres (c) as the cadres (c) are to relate to the masses (1/m). Or, in contrast to Rummel's parochial $P = aE + bC$, $E/c = c/1/m$

Thus $E = mc^2$

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1. Michael A. and Deena Weinstein, "Sartre and the Humanist Tradition in Sociology," *Sartre* (Mary Warnock, ed.; Garden City, 1971), 357-386.

2. Especially *Psychology and Epistemology* (New York, 971).

3. The hard-soft dichotomy as in tough minded realists and soft-headed idealists, as in hard knowledge and soft knowledge is in need of critical dissection, perhaps from a feminist perspective. One does not have to be a Freudian to understand the symbolic meaning of hard nosed.

3a. Michel Oksenberg, *China: The Convulsive Society*, Foreign Policy Association, December 1970; A. Doak Barnett, *Our China Policy*, Foreign Policy Association, February 1971.

4. Boardman's neglect of the facts permits him to be unfair to all sides. Contrary to Boardman's contention that no one tested the hypothesis of a link between domestic and foreign policy for China, Dwight Perkins and Morton Halperin did and found it wanting. Contrary to Boardman's contention that no one thought Chinese might perceive themselves as threatened, that idea was central to numerous studies from Allen Whiting, *China Crosses the Yalu*, written some 15 years ago, to Leon Sigal, "The 'Rational Policy' Model and the Formosa Straits Crisis," *International Studies Quarterly*, 14:2 (June 1970), 121-156. Contrary to Boardman's contention that "Great care was naturally taken to ensure as little contact as possible was made with social science," as the funding of the Navy's Project Michelson indicates the opposite is more nearly correct. Contrary to Boardman's contention that CCAS has neglected the social sciences, early on Jim Peck criticized the notion of modernization, the central social science concept used in comprehending contemporary China.

5. Boardman might better argue that much could be learned from some of the systematic social science studies of Chinese foreign policy – Peter Van Ness, Robert Keohane, Tom Robinson et al. – but I would question, for reasons to be made clear below, whether their good results flowed from the models applied. To the contrary, the good results permitted them to utilize a model to make those results *seem* persuasive.

6. Rosenthal and Fode, "The Effect of Experimenter bias on the performance of the albino rat," *Behavioral Science*, 8 (1963), 183-189. Rosenthal and Jacobson, *Pygmalion in the Classroom* (New

York, 1968). Neil Friedman, *The Social Nature of Psychological Research* (New York, 1967). Cf Liam Hudson, *The Cult of the Fact.*

7. For example, Maurice Freedman, "What the Social Sciences Can Do for Chinese Studies," *Journal of Asian Studies,* 23:4 (Aug. 1964), 523-530; Howard Boorman, "The Study of Contemporary Chinese Politics: Some Remarks on Retarded Development," *World Politics,* 12:4 (July 1960), 585-599; Chalmers Johnson, "The Role of Social Science in China Scholarship," *World Politics,* 17:2 (January 1965), 256-271. The list could be extended.

8. A more French and structuralist approach to this matter might follow Lévi Strauss, "Rousseau: The Father of Anthropology," *UNESCO Courier,* 16:3, pp. 10-14, through the writings of Gilles Deleuze and Michel Foucault. For American variations of these themes, cf. Eric Wolfe, "American Anthropologists and American Society;" Stanley Diamond, "Anthropology in Question;" Bob Scholte, "Toward Reflexive and Critical Anthropology," in *Reinventing Anthropology* (Dell Hymes, ed.; New York, 1973), 251-263 and 401-457. For a more German and Marxist effort based on works such as H. L. Nieburg, *In the Name of Science;* Jerome Ravetz, *Scientific Knowledge and Its Social Problems:* cf. Thomas Ferguson, "The Political Economy of Knowledge and the Changing Politics of Philosophy of Science," *Telos* (Spring 1973), 124-137. Floyd Matson, *The Broken Image,* and Theodore Roszak, *Where the Wasteland Ends,* relate directly to American social science concerns.

9. A good introduction to the Kuhn-Popper debate is Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge* (Cambridge, Eng., 1970).

10. Although I believe systems theory – to take one last school which Boardman mentions – is more a model than a paradigm, its translation of cybernetics into social science has been adequately comprehended as "Crypto-Totalitarianism," as a model "to bolster up the powers-that-be, and to indoctrinate the public with a mood of undiscriminating submission." (S. Andreski, *Social Science as Sorcery* [New York, 1972], Ch. 13) Asking "Is There a Paradigm in the House?", John Weltman (*Orbis,* 16:4 [1973], 1043-1056) writes of systems theory:

> Change in scientific disciplines is as much sociological as logical. A new approach takes over through the demise, or the silencing, of its opponents rather than conversion . . . academic concerns shortly came to reflect these areas of interest for policymakers . . . Much of man's understanding is conveyed through metaphor. Modern man's metaphors are often his own artifacts. In this case [systems theory] the metaphor is from cybernetics . . . only in the limited sphere of scholarly rhetoric may the systems approach be expected to have any long-term currency.

In sum, I support the intelligent use of social theory. Using such knowledge intelligently suggests, however, that Boardman is promoting something else: short-term rhetoric with appeal to ruling groups.

Rejoinder

by Robert Boardman

First, let me clarify: social science of the kind I described—"American social science" if you like—is not monolithic. Foreign policy scholars working in this tradition do, more than occasionally, disagree with each other on particular issues as on more fundamental questions of approach. I can only therefore give a personal reaction to what seem to me to be some of the features of the Chinese foreign policy literature. I would hope, though clearly I could not guarantee, that others working in the same tradition would find some common ground in my argument. By "others working in the same tradition" I refer not so much to the tightly in-bred field of China studies as to scholars working to develop the social science of International Relations, including foreign policy analysis. Friedman's comments on some of the quantitative studies that have appeared are in my view perfectly justified: my point was that the field of Chinese foreign policy studies has still to make effective contact with the world outside. Perhaps I could group my responses under four headings.

1. Social theory is undeniably richer than I had space to indicate in the paper. Its point was not to do a *tour d'horizon* of current ideas, but to argue that students of Chinese foreign policy, as far as we can judge from their writings, have tended to assume that such theorizing could shed little light on their subject. I emphasize the word "writings": it may be that scholars are deeply immersed in theory but feel inhibited at letting this influence their research, perhaps out of deference to the editorial policies of some journals in the field. If this is the case, my comments were misplaced; but I would like to see some evidence.

To take two of Friedman's examples, I would agree that Laing and Lévi-Strauss could indeed help us out in the task of theory-building, and have made this point elsewhere.[1] It may be, also, that some of Lewin's writings, for example on an individual's lack of cognitive structure,[2] could lead on to important insights into China's relations with other states. Chomsky poses more difficulties. He is himself persuasive about the common thread running through his work on generative grammar, his critique of Skinnerian method, his drawings from thinkers like von Humboldt, and his critique of official United States thinking on cold war questions. Without in any way being a specialist, I think I could sense a way in which insights from the first strand could be used in a study of the sinification of Marxism-Leninism, or of the evolution of Mao's early thought. But this begs a distinction which presumably Chomsky—and also Friedman—would hesitate or refuse to make, between the more "academic" and the more "political" sides of his writing.

We can throw theories at one another till the cows come home. Eventually we have to frame propositions that can be tested against data. It is this step that is the crucial one. Social science, as I understand it, is indifferent to the sources for the antecedent steps. It might be useful, for example, to extract from Mao's theoretical work a precise definition of a "contradiction" in a society. As a social scientist, I would be interested in going through some of the critical literature that Friedman cites: not out of a feeling that Gittings, Maxwell, Clark, and others have a clearer conception than most of the pervasive effects of the machinations of the United States officials, but simply in order to arrive at an inventory of propositions that can be articulated and tested. In these three, for example, I would expect to find the material for several groups of hypotheses relating (a) Soviet, Indian, United States, or other countries' actions, (b) Chinese perceptions of these actions, and (c) Chinese foreign policy behavior. In Mao I would expect to find the material for similar groups of hypotheses relating the external behavior of states to internal conflicts. All that social science claims is that procedures have been developed which it is useful to exploit when trying to assess the validity of such propositions.

2. My discussion of the more traditionalist literature rests, of course, on a caricature. This was designed quite candidly to emphasize the weaker rather than the stronger points in this body of literature. I do not deny that these

strong points exist. It would be worse than churlish to give a cavalier dismissal of the scholarship of a Needham or a Purcell or a C. P. Fitzgerald. However, to revert to Rosenau's analogy, the social scientist does not happen to be playing that particular game. And if we turn to those writers who emphasize the totalitarian features of Chinese society in their explanations of Chinese foreign policy, the point made above still applies. Totalitarian theory has potentially testable features, as does Freudian theory (is there a correlation between certain types of mother-child relations and certain types of adult syndromes?): but in practice its exponents have tended to draw back from taking this plunge.[3]

The second objection is derived from what I take to be a central assumption of the international relations literature, that is, that no explanation of foreign policy behavior is adequate unless it takes account of systemic as well as of national and sub-national variables. In the case of China, an overwhelmingly preponderant emphasis has been given to the internal (and more particularly to the governmental) sources of foreign policy. When reference is made to systemic variables, it tends to be in the specific form of constraints: as, for example, when the United States is held to have "deterred" China from aggressive acts in Asia. The major contribution of the radical or critical literature is its renewed emphasis on systemic factors. The points which Friedman makes in connection with McClelland's study, and later in connection with such activities as Chiang's attacks on the mainland in the early 1950s, the Nationalist blockade, Dullesian threats of massive retaliation, and so on, are well taken. I would agree that these and related events are crucial to understanding China's foreign policy in that decade. But this observation ought to lead on to the development of interaction models which could explore variables in the U.S.-China relationship further, for example through a parallel study to that on U.S.-Soviet interactions by Gamson and Modigliani.[4] Similarly, I would want to take issue as much with Friedman's placing of Mao at the fulcrum of Chinese policy-making, as with those writers who would see Mao as an obstacle to an improvement of Sino-American relations. Arguments about the role of Mao can make little headway unless they make at least some reference to the debates among political scientists and foreign policy scholars about the role of personality, role, governmental, and related variables, in policy-making. And on the Cultural Revolution, why not tie the analysis in with the theoretical and empirical literature on social conflict in the work of Coser, Gurr, and others?

3. Friedman seems to object to the "Americanness" of the social science tradition to which he is opposed. It is true that this aspect is often apparent, particularly in those areas that touch on questions pertaining to United States national security policy. But the comment is an irrelevant one, in the same way that it would be irrelevant to criticize Marx on the grounds that his theorizing reflects his feeling of Jewishness; or to criticize radical China scholars on the grounds that their work reflects concerns common to certain sections of American society. Presumably the sociologist would assume, almost by definition, that all knowledge is in some way rooted in particular cultures at particular times, even if its content is not wholly determined by these parameters. This applies to broader societies, as Northrop[5] and others have emphasized, and to more narrowly-defined academic sub-cultures, which is the starting-point of Kuhn's observations. However, the conception of knowledge in social science is not a platonic one. The question is rather: what procedures can we adopt so as to get as near as possible to the asymptote?

4. There are many critiques of American social science. To add to Friedman's array, one of the most devastating phrases is Toynbee's reference, in another context, to the "idolization of an ephemeral technique," which he associates with the nemesis of creativity in civilizations experiencing breakdown.[6] There are now a sufficient number of works critical of academic sociology, for example from Dahrendorf's *Homo Sociologicus* of 1958, to give the would-be quantifying Chinese foreign policy scholar cause for serious reflection. One thinks of Sorokin's analysis of the various diseases affecting sociologists, from "quantophrenia" to the amnesia that brings them to believe that nothing of merit has been written until the period of their own researches.[7] But serious reflection, I would submit, is at present a sufficient as well as a necessary response on the part of the China scholar. The grounds for refusing to embark on the social scientific path of conceptual refinement and hypothesis testing do not yet exist. Perhaps one day they will, if research becomes prone to quantitative faddism and the propagation of trivia. Friedman might want to retort that social scientists would be incapable of recognizing such a situation even if it was staring them in the face. I would give them more credit than that. But I would add that social scientists need their critics. There is, unfortunately, little evidence in Friedman's comments that radical scholars sense a need for them too.

Notes

1. See my "Conflict in Western Perceptions of Change: Two Profiles of China," *British Journal of Political Science*, I (1971), p. 208; and "Perception Theory and Chinese Foreign Policy," in Dial, ed., cited earlier. On Laing, I am thinking in particular of his *Interpersonal Perception* (with H. Phillipson and A. R. Lee, New York: Springer, 1966), which includes an extrapolation to the inter-state level of models of communication developed at the individual level. Perhaps Friedman would agree that his *Knots* (Pantheon, 1970) provides one of the best forms in which to summarize Washingtonese commentary on China!

2. In general, See K. Lewin, *Field Theory in Social Science* (New York: Harper, 1951); and the discussion by Morton Deutsch, "Field Theory in Social Psychology," in Gardner Lindzey and Elliot Aronson, ed., *The Handbook of Social Psychology*, 2nd ed., Vol. 1 (Reading, Mass.: Addison-Wesley, 1968), esp. 431.

3. Others have, though. See Andres D. Onate, "A Research Note into the Conflict Interactions of the People's Republic of China, 1950-1970," University of Arizona: Dept. of Oriental Studies (1973). Onate investigates the relationship between domestic and foreign conflict behavior in China's case and finds a moderate relationship between the two. His findings, however, seem to disconfirm the "totalitarian" assertion that Chinese foreign policy is a reflection of Mao's goal of consolidating the regime, by indicating that the relationship is the opposite way round: that variations in domestic conflict tend to *follow* rather than precede variations in external conflicts.

4. William A. Gamson and Andre Modigliani, *Untangling the Cold War: A Strategy for Testing Rival Theories* (Boston: Little, Brown, 1971).

5. See for example the contributions to F. S. C. Northrop and Helen H. Livingston, ed., *Cross-Cultural Understanding: Epistemology in Anthropology* (New York: Harper and Row, 1964).

6. Arnold J. Toynbee, *A Study of History*, Vol. 4 (New York: Oxford University Press, 1962), 423 ff.

7. Pitrim Sorokin, *Fads and Foibles in Modern Sociology and Related Sciences* (Chicago: H. Regnery Co., 1965).

Surrejoinder

by Edward Friedman

Boardman now says that my critique of mainstream quantitative studies of Chinese foreign policy is "perfectly justified," yet he continues to argue in the same old unjustifiable manner. He continues the quest for timeless, placeless truths which all can agree to, a quest which both Marx and Weber long ago pointed out was impossible in a world of conflictful social relations where perception cannot be divorced from social perspective. The data does not gather itself, pattern itself or explain itself.

If mainstream social science has proved less accurate than critical scholarship, then, Boardman suggests, it is critical scholarship's "propositions that can be tested against the data." That is, "an inventory of propositions" is prepared and social science procedures are used to "assess the validity" of the propositions. These procedures, Boardman claims, place the propositions within "systematic" "interaction models" including "national and sub-national variables."

Social scientists as diverse as Barrington Moore, Jr., Alasdair MacIntyre and Charles Tilly continue to point out, however, that the meaning of an isolated proposition changes as its context changes. Boardman's approach simply cannot explain most of the variance. It cannot explain why appeals from Chinese ruling groups to their counterparts in the U.S.A. for aid against threats from the north mean such different things in 1915, 1931, 1948 and 1970. The international world itself changes, as do particular dominant institutions, decisive belief systems and distinct socio-historic settings, all of which interact in a complex manner so as to give different meanings to the same propositions.

No rule can tell us how to comprehend the isolated proposition. One must be open to changes in patterns, to new realities and possibilities. Perhaps Benjamin Schwartz's notion of liberal totalitarianism, which he introduced in his study of Yen Fu, can help us understand why some mainstream social scientists have such an authoritarian attitude toward truth, cannot stand ambiguity, and insist on a mechanical answer to problems of social knowledge.

The ethically and intellectually rooted approach of critical scholars which suspects the "truth" of social givens is more open to this flux of changing and potential truths. If mainstream social scientists continue to argue that they and they alone divorce their objective scholarship from their emotional politics, if they must dismiss critical scholarship as mere politics, then they cannot critically comprehend why the

The point is not that critical scholars have a monopoly on social truth. All serious scholarship, whatever the political perspective, will have much to teach. The point is that there is no single road to truth. The mainstream social science effort to insist that its way is the only way masks the (totalitarian) politics which its scholarship of necessity contains. That politics has been revealed in its assertions about Chinese foreign policy as bad scholarship. At a time when university departments are so careful about hiring or promoting good critical scholars, it seems almost criminal for Boardman to charge that critical scholars are not open to dialogue and debate. This exchange, as much else in the *Bulletin*, shows that here as elsewhere with Boardman the opposite is much closer to the truth.

heroes of Boardman's original essay turn out to be apologists for various dominant political groups. Surely the foundations will not fire their advisors who argued that irrational Mao would never deal with the U.S.A. How much is the issue truth, how much power to ignore or impose truths?

There was a time when the notion of objective social truth played a liberating and humane role in negating the notion of revealed truth which protected narrow privileges. But today, unless one is very careful, that notion of objective social truth masks administrative manipulation and truths imposed by powerful bureaucracies.

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Financial Aspects of Chinese Planning

by Roland Berger

Economic policy in China is designed to serve the general political line.* In their talks with me in April 1973, members of the Budget Bureau of the Ministry of Finance made it unmistakably clear that financial considerations are subordinate to economic ones, which in turn are determined by politics.

"China's financial policy," they said, "is framed to help consolidate the dictatorship of the proletariat. Finance must play an active role in bringing about the socialist transformation of the economy by mobilizing funds and correctly handling the balance between consumption and accumulation."

Thus, in the Chinese view the economy determines financial policies although whether these policies are good or bad can influence the economy. Increases in financial resources must be found through expanding production of industry and agriculture and mainly by "reliance on our own efforts."

Budgetary policy, therefore, rests firmly on the actual output of agriculture and industry. Planned expenditure must be balanced in relation to national production, which determines budget income. In fact, the plan provides for a marginal surplus of income over expenditure, proving some leeway (*yü ti*) for contingencies. If a shortfall in income above this margin should arise from unforeseen circumstances such as seriously adverse climatic conditions, the solution would be sought in a special appeal to the people to practice economy and increase output. The Budget Bureau insists that expenditure is never met by printing paper money.

There is clearly no place in this scheme of things for the Keynesian remedy of gradual and eventually cumulative inflation or devices such as bank rate manipulations, stop-and-go techniques, off-and-on price controls (which at best are only temporarily effective), and other nostrums to which the capitalist world resorts, albeit without much confidence that they will succeed in halting inflation for any length of time. It seems, however, that there were those in China in 1962 who advocated such methods in dealing with the economic problems of the "three difficult years." Liu Shao-chi, we are told, would have had recourse to deficit budgeting, allowing the currency to "find its level" which would have resulted in a huge increase of money in circulation and the probability of serious inflation. His notions clearly did not find acceptance.

China, whilst giving aid, does not rely on foreign loans for investment funds. Loans received from the Soviet Union to help reconstruct the economy after Liberation and to back China's aid to the Democratic People's Republic of Korea to resist American attacks represented no more than 2 percent of State revenue for the years 1950 to 1959. These loans, said to be of the order of 1,406 million roubles, were all repaid at the beginning of 1965, one year ahead of time. Taking the standpoint that she has an obligation to assist other developing countries, China does not brag about her foreign aid. Firm figures are, therefore, not available. However, a director of the Schweizerische Bankgesellschaft, Zürich, recently expressed the opinion that the total is little short of $2 billion. It is almost certainly true that China has given more in terms of unrequited aid than it ever borrowed in foreign loans.

From 1950 to 1958, six series of national bonds were issued to utilize idle money, chiefly in the hands of the national bourgeoisie, to expand production. These totalled in value 3,840 million yuan. With the strengthening of State finance they were all paid off with 980 million yuan interest. Today it may be said that China is the only country in the world which is free from both internal and external debts.

In her foreign trade transactions China similarly aims to maintain an overall balance, with some excess of exports over imports. During the period of Soviet aid (1951 to 1955), China's imports exceeded her exports. In the seventeen succeeding years (1956 to 1972), imports have exceeded exports in only three years (1960, 1967 and 1970) and these by relatively small amounts.

Loans play no part in financing China's foreign trade. When high-cost items (e.g. airplanes) and complete plants are being purchased, she asks no more than normal commercial credits (three years in the case of aircraft; five in the case of complete factories). This contrasts with Soviet policy.

* This essay first appeared in French in substantially this form in *Le Monde Diplomatique* (August 1973).

According to the *International Currency Review* (July-August, 1973), by 1980, debt service is likely to absorb one half of the U.S.S.R.'s export earnings.

According to U.S. sources China's foreign exchange of gold reserves increased from $415 million in 1960, to $720 million in 1972 – $560 million in gold (from purchases and domestic production) and $160 million in foreign exchange.

However, the Swiss banker already quoted considers these estimates altogether too low. Taking account of foreign trade surpluses over a longer period (1950 to 1972) and with a "conservative" estimate of $500 million from remittances from overseas Chinese and other invisibles, he regards a currency reserve figure of $4 billion as completely defensible."

There is no personal income tax in China. State-owned (all people's) industry provides revenue in two forms: profit and tax. Commerce pays a tax based on turnover and the communes are responsible for the agricultural tax. With the expansion over the years of State-owned industry, the proportion of revenue from this sector has progressively increased.

In 1950 State industry and the cooperatives accounted for 34.3 percent of State revenue, capitalist industry and commerce 32.9 percent, and individual peasants 29.6 percent. By 1960, the last year for which we have a sectional breakdown, the percentage of revenue from State-owned industry had risen to 64.7 percent; industrial and commercial taxes 28.9 percent, and payments from the people's communes in the form of agricultural tax and taxes on rural industry and commerce 5.9 percent. The Budget Bureau state that today rather more than 90 percent of revenue is derived from the State sector.

Industrial enterprises pay tax on ex-factory prices, commercial departments pay a "business tax" (usually about 3 percent on the volume of business), and services such as hotels and laundries pay a tax of from 3 to 15 percent on the basis of income.

A large part of the budget (60 percent in 1972) is devoted to support production and capital construction. Investment funds for major projects of capital construction, such as the Wuhan iron and steel complex, are totally provided by the State. Medium-sized construction works may be funded by both the State and province in agreed proportions. A significant part of China's capital accumulation, however, is not particularly quantifiable. Edgar Snow in 1971 reported an interview with Chou En-lai.

> *The premier put agricultural product value for 1970 at about 25 percent of total combined output value of industry, transportation and agriculture. Calculated on that basis, China's industrial-transportation-agricultural output value in 1970 was around 120 billion American dollars. There is, however, no reliable index for converting "output value" in China in terms of gross national product systems used in the West. "Output value" omits or minimizes important "service values" such as rents, privately owned homes – now rising by the millions, built with mutual labour – as well as major and small water conservancy projects constructed by army and volunteer labour. Or consider China's nationwide urban and rural air-raid shelter system, built largely by unpaid neighbourhood mutual labour teams. It would elsewhere cost billions of dollars*
>
> *Finally, how is one to fit into the GNP formula the value of some 30 million acres of marginal land added during the past ten years to the cultivable area by peasant labour – with unbelievable toil – at a cost of little more than peasant food consumption?**

Some measure of the effect of output of investment policy may be gauged from figures given to me by the Planning Commission of Hupeh province (population 38 million) in May 1972, where the value of industrial output of 7,500 million yuan in 1970 had been increased by 1,000 million yuan in 1971.

Doubts have been cast on the economic effects of the Great Leap Forward (1958). Assertions that the economy was set back at least ten years by the excesses of this period have now given way to more realistic assessments. In a submission to the Joint Economic Committee of the United States Congress, Robert Michael Field comments:

> *. . . in 1958 alone, many new construction projects were started, and 500 were completed. Merely putting these new plants into operation would have been enough to guarantee China substantial gains in industrial production. Thus the political excesses of the period masked what was a truly substantial achievement in expanding industrial capacity." [compendium of papers, May 18, 1972]*

Support for the Rural Population

Perhaps in no area of the economy have China's achievements been so strikingly successful as in their handling of agricultural collectivization and, with special emphasis since the Cultural Revolution, in raising the standard of living in the countryside to bring economic and social conditions in the rural areas closer to those of the towns.

Over the years, the weight of the agricultural tax on the rural economy has been consistently reduced. The tax, which is levied on output of basic grains, oil-bearing seeds, cotton and tobacco, represented 13.2 percent of production of these items in 1952, was reduced by 30 percent in 1962, and today averages for the country as a whole about 6 percent. If, owing to adverse weather or other conditions, production falls, a proportion or the whole of the tax is remitted. As we have seen, the percentage of budget income represented by the agricultural tax has been falling from year to year.

Each commune receives a quota under the economic plan of basic commodities which the State will purchase at fixed prices. Above the quota surpluses of grains can be sold to the State at prices 20 percent to 30 percent above this fixed price. As the quotas remain unchanged for a period of five years (the last quotas were fixed in 1971), any increase in production can bring in substantial additional income to the accumulation funds of the production brigades and teams.

* From one perspective, GVO (Gross Value of Output) *under*states GNP because, as Chou notes, it excludes services, unpaid voluntary labor, rents, etc., all of which become part of GNP in the West, except for unpaid labor. In another sense, GVO *over*states GNP because of double-counting certain items of industrial production, e.g. coal counted once when mined, again when used to make steel, and again when steel is used to make other products. (The Snow interview appeared in *Epoca*.)

In determining the scale of the tax, grain is taken as the standard and other items are assessed in grain equivalents. For example, in 1972 100 catties of tobacco was taken as equivalent to 50 catties of grain. A tax, again calculated on the basis of grain equivalents, is paid on items such as fish, silk, vegetables and fruit, which are sold to the State at fixed prices. The agricultural tax is collected by the county, which retains 5 percent for its own use and passes the rest to the State.

Of major benefit to the peasant population has been a consistent policy to close the gap between the prices paid to the communes for agricultural supplies and those charged for industrial products needed in the rural areas. The latest cut in prices of industrial goods took effect in two stages in January and July 1971. Prices of tractors and diesel engines, petrol and lubricating oil were reduced by 20 percent; fertilizers and insecticides, plastic sheeting and piping by 16 percent. At the same time, prices paid to the communes for sugar cane, oil-bearing seeds, some fiber products and various sideline items were raised. The price paid to the growers for cotton was raised in 1955 and again in 1964. Together, these amounted to a 25 percent rise. The purchase price of grains has been raised several times since 1949 without increasing the sale price to the consumer, and all seasonal differentials in grain prices have been eliminated.

Professor Joan Robinson has remarked that:

> *... when the purchase price of grain was raised to encourage production during the bad years, the retail selling price was kept unchanged. This avoided the vicious cycle of inflation which is inevitable when food prices rise, so that urban wages have to be raised, which raises money demand for food and so on round.* [Economic Management – China 1972]

A Chinese report in September 1972 stated that the closing of the "price scissors" had benefitted the peasants, either by increasing their income or by saving expenditure by some 10,000 million yuan in the period since 1966. At the Hsin Chou county (Hupeh province) which I visited in May 1972, I was informed that this one county, which had paid for industrial goods before the 1971 cuts were announced, had received back no less than 200,000 yuan.

The State, in fact, loses on its purchase of a number of items from the communes. For example, rapeseed, which is purchased at 81 cents a catty, is sold at 78 cents, the State bearing the cost of handling and other overheads in addition to the 3 cent loss per catty. Rice bought at 16 cents a catty is sold at 14. With transportation costs, the State loses 2.8 cents on every catty.

The People's Bank offers facilities to the communes to borrow at low rates of interest. For the purchase of farm machinery, interest is charged at 0.18 percent a month (2.16 percent per annum) and for farm production (seeds, fertilizers, and so forth) at 0.36 percent a month (4.32 percent per annum). As these loans are mostly for seasonal requirement it is customary in China to quote interest rates on a monthly basis.

These price adjustments reflect one aspect of price policy which the Chinese describe as "exchange on the basis of equal value," although in some cases industrial goods are actually sold to the communes at below-cost. Members of the Budget Bureau gave me examples of how these adjustments in price relations had affected the peasants' sales and purchases. In the early period after Liberation one catty of salt was exchanged for several dozen catties of grain. Today, one catty of salt could be exchanged for one catty of grain. One tael of gasoline twenty years ago cost the equivalent of three eggs – today the ratio is two taels for one egg. One injection of penicillin, which cost 50 catties of flour, was now the equivalent of one catty.

The cumulative effects of the various measures to benefit the rural areas have obviously played a major part in the rapid development of Chinese agriculture. They have also greatly augmented the communes' accumulation, as well as the income of individual peasants. According to the Budget Bureau, commune savings in 1971 were double those of 1965, and the private savings of commune members increased by 30 percent in the same period.

Acting on the principle of local self-reliance but operating within a centralized plan, the counties (the level above the people's communes) are developing small and medium industries which are steadily increasing the counties' accumulation funds.

Hsin Chou county (Hupeh province, population 690,000), for example, which I visited in May 1972, had set up 120 factories since 1965 with a total output value in 1971 of 39,010,000 yuan (based on ex-factory prices). Investment in machinery in the seven-year period amounted to 27,500,000 yuan. In addition, this county had received part of the amount by which quotas had been overfulfilled. The quota for the county of 10 million yuan had been exceeded by one million yuan, of which the county retained 65 percent, passing 35 percent up to the administrative region. And, as already mentioned, the county retained 5 percent of the agricultural tax collected from its 67 communes. With these funds, plus with a percentage of profits on its commerce department, the county is able to invest in new industries, develop capital construction projects such as water conservancy schemes, provide social services or contribute to the costs of those run by the communes, and cover its own administrative costs.

The total value of sales through the county's commerce department was running at some 61,000,000 yuan a year. The volume of sales and purchases through the free markets, which operate within rather strict controls, represented 2.9 percent of the county total.

The communes similarly dispose of sizable investment funds. At the Yueh Chi commune in the Wu *hsien* (county) near Suchow (Kiangsu), admittedly in a fertile area with above-average prosperity, I found in April 1973 that mechanization of agriculture was fairly advanced as a result of purchases of machinery and equipment in recent years from the commune accumulation fund.

This commune had purchased two 35 h.p. tractors; 67 walking (12 h.p.) tractors (80 percent of cultivated land was tractor-ploughed); 11 irrigation and drainage stations; 12 electric transformers; 15 electric motors (284 KW capacity) for irrigation pumping; 71 pumps; 18 semi-mechanized threshing machines; 183 other threshing machines for wheat and rice; 15 mechanical, and 178 compressed-air sprayers; 2 rice-husking machines, 11 wheat milling machines; 56 rice transplanters; 19 4-ton motor-powered cement boats and 520 other cement boats; 12 trailers; and 42 handcarts. The commune had equipped a farm implement repair and maintenance shop with lathes, drilling, planing and electric welding machines.

This commune derived 1,900,000 yuan a year from 37 sideline occupations – including embroidery, fish raising, mushroom cultivation, silkworm breeding, and the growing of water plants used for fodder and fertilizer. This is a region interlaced with waterways along a section of the Grand Canal, hence the cement boats which are produced by each of the eight counties in the administrative region by a simple process of casting cement onto a wire-mesh frame. The bulk of goods are moved to supply centers and depots by means of this cheap form of transport.

There are clearly wide differences in the natural conditions – fertility of soil, water supply, character of the terrain – from one area of China to another, which might make for wide disparities in the relative prosperity of the 50,000 people's communes. To avoid a situation in which the rich communes get richer whilst the poorer remain poor, a policy is operated to preserve a nice balance between local self-reliance and assistance from the State. There are production brigades – Tachai and Shashiyu are the outstanding but not the only examples – where peasants have succeeded by their own efforts against the most adverse conditions without calling for State assistance. Generally, the adjustments in the application of the agricultural tax and quota systems make allowance for communes which have special difficulties, but where assistance in this form is not adequate, direct financial support is provided by the State. Substantial amounts were granted after the formation of the communes in 1958 to give the poorer units a flying start.

Funds are also included in the budget each year for water conservancy schemes, for the opening up of new land, for the introduction of higher-grade seeds and other measures likely to bring economic benefits either directly or indirectly to the less prosperous areas. The adjustment of the income and expenditure between richer or poorer provinces through the national economic plan is another method by which the pace of development of the poorer regions can be accelerated in comparison with those which, by reason of history or geography, are better endowed.

On the basis of expanding production, the budget can allocate increasing amounts to improve the quality and expand provisions for health, education, science, technology and cultural services. Here again, the Chinese strive to maintain a balance between State aid and local self-reliance, with its concomitant of grassroots democracy. The number of pupils in primary and middle schools in 1972, according to Budget Bureau figures, increased by 30 percent over 1965. Much of this increase would be accounted for by additional places in rural schools as a result of the campaign to provide at least five years' primary school education for children in the countryside. These schools are under the supervision of the peasants and the production brigades receive 120 yuan a year for each teacher, the balance of the salary coming from brigade funds.

Each of China's 2,000 counties has a hospital and every commune a clinic; 60 percent of the salaries of doctors in commune clinics is paid by the State (through the county). The rest is provided through the cooperative medical service for which peasants contribute one yuan a year per head. All social services for industrial workers – health, pensions, crêches, kindergartens – are provided free of charge through the factory welfare fund (11 percent of the total wage bill) which is provided by the State. Family members of workers pay half the cost of medical treatment, although in cases of special need funds can be made available from the factory welfare fund.

Financial assistance to the minority peoples the Budget Bureau explained, is provided within the context of the party's policy of "unity, mutual help and love." The minority areas are given much greater control over their own budgets than the purely Han provinces. Yearly expenditure in the national economic plan provided for the minority regions greatly exceeds planned revenue, in contrast with the richer provinces where the reverse is the case. The State, moreover, devotes large sums every year to investments in capital construction projects especially for these areas and also provides non-repayable subsidies for the extension of social and cultural services.

"Every year, when preparing the budget," the bureau members informed me, "we have to think of the obligation of those who have already been liberated to help others in their just struggles. Whilst building socialism in our country, we endeavor to support the revolutionary struggles throughout the world. But China is a developing country and we are conscious of the fact that whatever help we can give to others is still rather limited."

The yearly economic plan includes a figure for expenditure and income for each province, municipality and autonomous region. The two are not connected and whereas planned income of the richer provinces and municipalities greatly exceeds their expenditure, the reverse applies in the case of the autonomous regions and the poorer provinces. For example, expenditure in the provinces of Kiangsu and Liaoning and the municipality of Shanghai was 33 percent, 18 percent and 10 percent respectively of planned income for 1972.

Prices of all major items, including most of the daily necessities, are fixed centrally with marginal differences depending on quality and with some small regional variations, which are gradually being eliminated, reflecting costs of transportation. Prices of some small items – towels, slippers, leather shoes, pots and pans – are decided at the provincial or administrative region level where they are produced and consumed. These arrangements do not exclude some flexibility to avoid waste. When, for example, there is a glut in one area or another of fish, fruit or vegetables which cannot all be processed or stored, prices are temporarily reduced and the items are sold at a loss to the State, although the prices paid to the commune remain stable.

Apart from the price adjustments already mentioned of benefit to the peasants, cuts in the prices of a wide range of items purchased by both town and country dwellers have been introduced as productivity has increased. Reductions in the prices for the most-used medicines and drugs in August 1969, for example, brought the overall price down by 37 percent (or 80 percent below 1950).

In 1972 the effective purchasing power of the people in town and country rose by 10 percent, which was reflected in a larger value of savings and increased purchases. In 1972, sales of piece goods of synthetic fibers rose by 30 percent, sales across the country of thermos flasks, aluminum cooking pans and other daily necessities increased from 10 to 20 percent, and there was a marked rise in purchases of the more expensive items such as radios, silk materials, wrist watches and bicycles.

Many visitors to China have remarked favorably on the price, quality, range and design of consumer goods offered in the large urban department stores as in well-stocked village

shops run by production brigades. Professor J. K. Galbraith, who visited China towards the end of 1972, has this to say:

> *The Chinese seem to have developed a plain but remarkably efficient system for the distribution of consumer goods. The markets and stores we examined were handling a dense traffic at markups that are most miniscule by our standards — 4.4 percent on fresh produce and meats, 13 percent in a big Peking department store. The department store is helped by having no packaging, no high-end, slow-moving items, no advertising, no style-goods, no fitting, few returns, no shop-lifting.*

Chinese factories producing goods for public consumption go out of their way to discover the buyers' likes and dislikes. When visiting the Changsha (Hunan) Rubber Boot and Shoe Factory in the autumn of 1971, I found fifty workers from the shop floor were out on the communes in the province studying foot sizes, suitability of shapes, and durability for work in the paddy fields, and generally to obtain the opinion of the consumers of the factory's products. They were interviewing some 4,000 peasants. On their return they would report to their workmates and modifications would be introduced in future production. Most factories producing consumer goods have adopted the practice of regularly sending some of their workers to the stores to sell their wares and at the same time to obtain an on-the-spot consumer reaction. Part of the practical training of economists studying at Peking University takes place in the department stores. Consumers in the West who often buy a new product rather than shop around to get an old model repaired will be interested to learn that in the shopping center in Tientsin, to serve the people and in the interests of frugality, seventy counters have been set up to repair fountain pens, wrist watches, lighters, shoes, knitwear, cotton clothing and enamelware.

Production is for use, not profit. The approach was explained in a statement by the Writing Group of the Ministry of Commerce (October 1970):

> *. . . we must actively handle commodities which the workers, peasants and soldiers like and which are economical, practical, unpretentious, durable and varied. At the same time, great attention must be given to making repairs. . . . We stand for the proletarian richness of variety and colorfulness . . .*

It would seem that the country which has talked least about material incentives and consumer sovereignty has, without a lot of fuss, been the most successful of either other planned-economy countries or of the Third World in increasing the quantity, extending the variety, and improving the quality of its consumer goods.

By planning for an effective balance to ensure the proportionate development of heavy and light industry and agriculture, by maintaining a supply of consumer goods to correspond with the purchasing power of the community, and by operating a price policy to serve the interest of the workers and peasants, China has since 1949 successfully avoided inflation.

This is not to say that conditions automatically excluded the threat of inflation on a number of occasions since 1949. In the three difficult years, 1959-61, floods, typhoons and drought played havoc with agriculture, drastically reducing the output of foodstuffs and the supply of industrial crops for light industry, which, at the same time, had an unfavorable effect on government receipts and expenditure. By a masterly policy of self-reliance and just and equitable distribution and thanks to the commune system, scarcity was fairly shared and starvation averted. On the purely technical level, the avoidance of inflation in these circumstances was an historic achievement which the majority of economists in the West seem to have ignored.

More recently a decision of the Central Committee to raise the wages of many of the workers in the lowest three grades of the eight-step scale clearly increased monetary income and could have contributed to a degree of inflation unless compensatory action was taken on the supply front. The ruling was made in August 1971 and factories were given time to effect the changes, but the increments had to be back-dated to July 1971. By the time the enterprises came to put the ruling into effect, workers received nine or ten months' increased pay. The injection of such a sizable sum of purchasing power into the economy (in some factories 30 percent of the workers benefitted) could obviously have caused a run on consumer goods with dangers of inflation. In fact, the problem had been foreseen and supplies of consumer goods were increased to meet the additional demand.

With stable prices, increases in money income are genuine, not spurious. The "English disease" (stagflation) is nowhere in sight.

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Political Imprisonment in Indonesia

by Carmel Budiardjo

In a world where so much is spoken and written about the problem of political persecution and in particular political imprisonment, it is a strange fact that so little is known about the case of Indonesia. Yet in terms of the number of people involved, the long-drawn out nature of the problem, the treatment inflicted upon the prisoners, and the refusal of the authorities to apply judicial and humanitarian standards, Indonesia presents one of the most pressing and desperate situations.

In numbers alone, it comes second only to South Vietnam; according to an estimate made early last year by Amnesty International, there are at least 55,000 people being held in political detention, a figure which most informed observers feel considerably understates the problem. The majority have been held for nearly eight-and-a-half years and all, with the exception of a few hundred, are being held without trial and with no prospect of ever being tried.

The problem dates back to the events of October 1965 when a coup attempt was quickly repelled by the Indonesian Army under General Suharto, now the country's president, and massive arrests of communists and alleged communists took place simultaneously with one of the most ghastly massacres in modern history, during the course of which at least half a million people lost their lives. The attempted coup was the work of a group of middle-ranking Army officers who wanted to pre-empt plans which they believed were being made by the Army General Staff to gain control of the country in time for the demise of President Sukarno, who was thought to be on the verge of a fatal illness.

The extent to which the Indonesian Communist party was involved in this coup attempt is still a matter for conjecture in many circles but within hours after the coup was repelled, the Indonesian Army strongly condemned the party for having engineered the coup and launched an onslaught against it and many mass organizations that were directly or indirectly under its influence. The Communist party and its supporting mass movement had, by the time of the coup, attracted a combined membership of many millions. All these organizations, involving trade unions, peasants, women, youth, students, artists, academics and scientists, were declared illegal, initially by local military commanders and subsequently by order of the central government. In the first wave of arrests about a quarter of a million people were taken into custody. Since those first horrendous days many have been released, but arrests have continued without stop and today a large number of those arrested during the days and weeks after the events of October 1st still remain behind bars, together with many thousands who have been arrested in the intervening years.

The arrests are based not upon any article of the country's code of law but upon extra-constitutional powers that have been vested in the all-powerful *Kopkamtib* (Command for the Restoration of Security and Order), a command which empowers military commanders at all levels of the Army hierarchy to execute orders on a multitude of political, security and social affairs in addition to their purely military functions. *Kopkamtib* was set up in 1965 for the purpose of "combatting communism"; by virtue of its powers, Army commanders throughout the country are authorized to arrest political suspects, to interrogate them and to decide whether they should be held in indefinite detention or, in the case of a very small minority, brought to trial. Although other sections of the armed forces – the Navy, the Air Force and the police – do on occasion make arrests, the vast majority are made on the orders of *Kopkamtib* enforcement officers. Thus the arrests are based not on warrants but on military orders, and in the vast majority of cases these orders are not presented in writing to the person about to be arrested, which only makes the process even more arbitrary than it might otherwise be.

Most of the detainees have been arrested because of their alleged membership, prior to the coup attempt, of the Communist Party or one of the other banned organizations at a time when these organizations were still functioning legally. The very expression used to describe the grounds for political arrestees is, moreover, indicative of the arbitrary nature of the allegations; a person is arrested because "there are indications of involvement in the 1965 coup attempt/Communist Party." Whether involvement is "direct or indirect" does not really matter because either allegation can lead to arrest and detention. The words "indications" and "involvement" have frightening connotations in Indonesia's political jargon today. Inevitably, in a situation where arrests are so arbitrarily set in motion, many people now being held as political detainees have no political past whatsoever but have been victimized as relatives or friends of persons being sought by the Army; they include people who have been unable to prove their identity to the satisfaction of the arresting unit, who have been persecuted as a result of personal difficulties with neighbors or co-workers or for a host of equally futile reasons.

Maltreatment and torture are commonplace, especially during interrogations which are carried out by special intelligence units of the Army. Torture by electric shock is one of the most common forms employed, as well as sexual torture, whipping and beating, and solitary confinement in cells with no access to light and too small even to lie down in. Although interrogators are generally instructed not to go so far as to kill their victims, there are a number of cases where detainees are known to have died during or as a result of torture. Little is known to the outside world about the bestialities to which Indonesian intelligence officers resort in their relentless search for "evidence," "confessions" and

incriminating information that would lead to the arrest of others. It is appalling that intelligence officers in Indonesia can proceed in their practices with impunity behind a wall of silence and ignorance.

Army intelligence officers like to make a distinction between the initial round of interrogations which they call "operational" and the subsequent interrogations, the so-called "judicial" round of interrogations. For officers in the intelligence corps, the distinction is that the "operational" interrogations are directly linked to their operations against political opponents, aimed at crushing all forms of opposition or resistance to the regime, whereas the "judicial" interrogations are intended to establish the "guilt" of the person concerned and to determine the future status of the detainee. This process has nothing whatsoever to do with the judiciary for it is handled entirely by military intelligence officers who make their decisions not according to the law but in accordance with the orders of their superior officers in *Kopkamtib.* Arresting and interrogation units do have powers to release the people they have taken into custody but there are very few releases. In the vast majority of cases the detainees are designated for transfer into the hands of a regional interrogation team, transferred to one of the many prisons where political detainees are being held and are classified into one of the categies. Once a detainee enters this stage of the process, there is no longer any possibility of an individual consideration of his case; at this stage the category is all-important and decisions are taken by category. It should be stressed, however, that a prisoner is rarely, if ever, informed of his or her category and it frequently happens that a prisoners' category is changed without his realizing it.

There are three main categories into which detainees are classified, A, B and C. When a few years ago the Indonesian authorities became somewhat embarrassed by international criticism and sought to assuage concern by proclaiming to release all "C" category prisoners, a new category emerged, the "X" category. The "A" category prisoners are those against whom there is, according to the military officers in charge, sufficient evidence to warrant a trial for "involvement." Until about a year ago, Indonesian government spokesmen generally seemed to concur that there were about 5,000 "A" prisoners, those awaiting trial. (Those already tried are no longer regarded as political prisoners and are therefore not included in this category.) In March 1973, President Suharto gave a completely new figure for the "A" detainees. He announced that there were 2,000 "A" prisoners. Four months later, in July 1973, he told a British journalist that there were 2,645 "A" prisoners (reported by Stewart Harris in the August 17 issue of *The Times*). President Suharto never sought to explain why the number of "A" prisoners had suddenly fallen from the earlier figure of 5,000 and it would be very wrong to conclude that several thousand had been tried. The trials proceed at a rate that is out of all proportion to the numbers awaiting a court hearing.

In June last year, General Sumitro, *Kopkamtib* Commander, told journalists that trials would be held at a rate of 200 a year. Few people believe that this rate can be maintained for the Indonesians themselves point to the acute shortage of judges to handle cases of any kind. But even assuming that Sumitro's pledge is kept, it will take another thirteen years to complete the trials of "A" category detainees, many of whom have been in custody since 1965. In other words, some will have been held in prison for as long as fifteen years before they can hope to be brought before a court of law.

Some trials are held before special military tribunals (*Mahmilub*) which deal with military personnel who are being held as political detainees and also some of the more prominent civilian cases. The other trials are held before ordinary courts. In all cases the sentences are marked by their severity. According to current estimates there have already been about fifty death sentences passed, some of which have already been executed while others are awaiting a decision from the president on clemency appeals. There have also been numerous life sentences, while most of the remainder range from eight to twenty years. The rate of acquittals has been less than two percent.

"B" category prisoners are those against whom there is no evidence of involvement in the coup attempt and who therefore cannot be brought for trial before a court of law, for to do so would result in their immediate acquittal and release. Nevertheless, the authorities regard them as being "dedicated communists," "traitors," "dangerous to state security" and therefore refuse to release them. This category comprises the main bulk of the political prisoners. In October 1972, an official spokesman announced a figure of 39,000 "B" prisoners, but five months later, in March 1973, President Suharto said that there were 13,117. Yet four months later, he told Stewart Harris of *The Times* that there were over 26,000 "B" prisoners. As the reader will no doubt already realize, little credence can be given to the figures published from time to time by the Indonesian authorities as they vary sharply from announcement to announcement and no attempt is ever made to explain the discrepancies. Anyone who has tried to wade through the tangled confusion of figures has quickly become familiar with the "numbers game" as it has come to be called. The former Attorney General, General Sugih Arto, himself reflected the cynicism of military officialdom when he said in reply to questions from foreign journalists two years ago: "We don't know how many political prisoners there are. The number floats, just like the rate of exchange of the Japanese yen vis-à-vis the dollar."

Whatever figure the authorities are currently supporting, it is clear that the main bulk of the prisoners are classified as "B" category; they cannot and will not be tried, nor will they be released. Ten thousand "B" prisoners have been transferred to a prison island camp on the island of Buru, where they live in total isolation from their families and society. In Plantungan, Central Java, a special camp for women "B" prisoners has been established. The government has indicated a number of times that it intends to set up more such camps for long-term detention, a frank admission that the banishment of many tens of thousands of politically undesirable people from society is meant to be permanent.

Having banished so many of these "B" prisoners from society and condemned them to a life of rigorous hard labor as the only means of their survival, the government proclaims that these prisoners will be permitted to enjoy "social integration" with the surrounding population. When "social integration" takes place, detainees will be allowed, so it is proclaimed, to have their wives and children with them and will be permitted to exchange their produce and participate in

other forms of economic activity. Despite all the talk about "social integration," as yet only 83 out of the 10,000 prisoners on Buru have been joined by their families; these wives, though technically free, are living under conditions of constraint that are hardly distinguishable from the conditions under which their husbands are being held. Recent attempts, following a visit of General Sumitro to Buru, to depict the situation of Buru detainees who have been joined by their wives as ideal have only disclosed the many difficulties now being faced even by the wives. Many of them, within a year of arrival in Buru, have had to sell what few possessions they were able to bring with them in order to buy many of the basic necessities that they cannot otherwise acquire as they are not allowed to earn or possess money.

"Social integration" is nothing more than a promise, and a very vague one at that. The authorities refuse to indicate when they intend to introduce it and they have also made it quite clear that, whatever it is, it will be granted only to those prisoners who are willing to undergo "a change in mental attitudes" and to confess to the "error of their ways." Such a requirement is not one with which the prisoners have the option to comply by means of a statement of adherence to the state philosophy. It is one that is left to the judgement of those in charge of the prisoners, based on their own assessment of the way a prisoner behaves and how he speaks. Needless to say, few prisoners would acknowledge that they have committed any errors, and even those who do so are not likely to be believed.

The "C" category is one that the government apparently wants to eliminate, for indeed, considering the definitions of "A" and "B" categories, there can be no further scope for yet another category. It covers all those whom the government describes as "hangers-on" or "fellow-travellers" who can be released because they are "less dangerous" than the "B" detainees. In late 1971, when the Indonesian government was anxious to placate international opinion, especially after the Dutch foreign minister raised the matter of the prisoners during a visit to Jakarta, it stated its intention of releasing all "C" detainees. Some releases were indeed reported in the press but these did not account for the more than 20,000 "C" detainees which, according to official figures, were being held at the time the pledge was made. In March 1973 President Suharto declared that 80,000 "C" prisoners were released at the end of 1971 and the beginning of 1972. This was clearly at variance with previous declarations which put the *total* number of prisoners at far less than 80,000! At the same time, the "X" category which covers those who are awaiting classification or reclassification suddenly expanded as did also the "B" category. In July 1973 Suharto mentioned a figure for this category of more than 7,000; clearly what has happened is that a large number of "C" detainees have been reclassified as "B"'s or placed in the "X" classification.

"C" category detainees who have been released face enormous problems in adjusting themselves and become the target of abuse and allegation whenever anything happens that might be described as an "act of sabotage." A particularly ugly example of how ex-detainees of the "C" category are treated was the incident in East Java where an ex-detainee was called to the local military unit and publicly beaten with an iron rod to intimidate peasants who were refusing to hand over their land for use by the state sugar company. Yet the man who fell victim to this brutal ploy had himself been the first to hand over his land, realizing how hard things would be for him if he did not.

The "C" category is a classification which applies not only to detainees but to persons outside who may never have been arrested but who have unfavorable political backgrounds. In 1973 all government departments announced, on the orders of *Kopkamtib*, that all "C" category personnel would have to be purged by the end of 1975. These would include all rank-and-file members of left-wing trade unions who somehow had never been discovered in earlier purges, or who had never been dealt with previously. The extent of this new threat is revealed by a press report from Semarang, Central Java, which stated that the Semarang State University would lose about forty-five people from its staff of lecturers and administrative personnel as a result of the purge. It is truly incredible to think that even now, nearly nine years after the 1965 events, people are still being victimized on such a scale merely because of their political affiliations before those events occurred.

For the prisoners themselves, who are permitted absolutely no recourse of legal counsel and who are not even officially informed of their classification, the categories have little relevance. Even if a prisoner has reason to believe that he has been classified into a particular category, he cannot therefore conclude that he is any the more or less likely to be released, or on the other hand tried, than the next fellow. Classifications can and do change quite arbitrarily, without any re-investigation or re-interrogation. There are numerous instances, too, of prisoners having actually been processed for release, with absolutely no follow-up nor indeed any explanation as to why nothing happened. Many of the women prisoners who were transferred to the Plantungan Camp for long-term detention had previously been processed for release.

The Conditions

In every town, large and small, throughout the country, there are prisons, interrogation centers, transit detention camps and military units where prisoners are held in a great variety of conditions. Even military units at the village level might from time to time be holding detainees in custody. Many of these places are completely unknown to the outside world and even to the local population, and it is virtually impossible to discover anything about conditions. Enough, however, is known about many of the prisons in Java and about a few of the detention camps and interrogation centers in some of the larger towns to be able to piece together a fairly accurate picture of conditions for the detainees in general.

Official allocations for food provide for the bare minimum: two plates of rice a day, a small dish of watery vegetables, an occasional piece of salted fish and a tiny portion of soybean cake. The prisoners hardly ever get eggs or meat and nothing in the way of beverage is provided except boiled water. No other food is provided, neither do the prisoners get any clothes, soap or bedding. They all sleep on the floor and are not even provided with mats. What they have in the way of clothes, mats and other requirements is provided by families or occasionally by church relief donations. Only a small proportion of the prisoners are in contact with their families and receive supplementary supplies with any regularity.

Contact with relatives is extremely difficult. In those cases where the families live nearby and have been able to keep in touch, the detainees may receive food and, occasionally,

visits. Prisoners are not, however, allowed to communicate with their families or even to inform them where they are. As a result, there are many who have had no contact with their families for years; husbands who know nothing about their wives, women who do not even know where their children are and youngsters who have no way of getting into touch with their parents. Detainees in Buru and Plantungan and a few other long-term detention camps are theoretically permitted to correspond with close relatives, but many letters never get through or take a year or more to reach their destination. All correspondence is heavily censored and may not exceed about twenty-five words. The detainees are also prohibited from reading newspapers or books; the only reading matter permitted them is the *Bible* and the *Koran*.

Tradition and Revolution in Vietnam

Nguyen Khac Vien

$2.65

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The conditions described above are those known to exist in some of the better-known places; in camps and prisons less accessible to outside contact, the conditions may be far worse. In particular, food frequently fails to come up to the bare minimum allowed for by the official allocations. Corruption and rising prices seriously reduce the amount of food made available to the prisoners. Inflation in Indonesia since the latter part of 1973 has led to a serious worsening of conditions, and even in the Salemba Prison in Jakarta, which is more accessible to supplies of food from families and from various relief projects, prisoners are known to be dying of starvation and suffering from acute malnutrition.

Future Prospects

The problem of political detention in Indonesia is almost nine years old, and there are no prospects that the Indonesian government intends to put an end to this gross and chronic violation of human rights. On the contrary, while on the one hand it talks of "social integration" in an effort to impress foreign opinion, it continues to arrest people for a variety of reasons. Still today, people are being arrested for their political activities prior to the 1965 events. In January 1974, following the emergence of a movement of dissent against current government policies, more than 750 people were taken into custody. Many of these are people who actively supported the military takeover in 1965 and 1966 but have since become deeply disillusioned with the government because of the widespread corruption and the glaring social injustices of economic development financed so heavily by foreign economic aid and capital investment. Far from solving the long-standing problem of political detention, the government is widening the scope and extending the whole problem to include people who are in no way connected with left-wing activity.

Political detention in Indonesia is part of a general repression of any form of political activity, coming from the right as well as from the left, that might endanger the tight grip of military control. There is virtually nothing that can be done in Indonesia to press for releases or even to urge that the government reconsider its attitudes; anyone who were to dare to do such a thing would certainly fall under suspicion as being "pro-communist" and would face the risk of detention himself.

Pressure from abroad can, however, play an important part. The military regime in Indonesia is very anxious to establish itself as an acceptable government and to assume a stature in Southeast Asian political developments and in world affairs. It relies heavily on foreign aid and investment, which is channelled to Indonesia through an international aid consortium, the Inter-Government Group on Indonesia. It has shown itself to be extremely sensitive to criticism that comes from any of these countries. The IGGI governments are, generally speaking, reluctant to take a stand on the human rights issue in Indonesia, for they too want to preserve their good standing with Indonesia. But if public opinion in these countries is properly informed of the political prisoner situation and expresses strong opinions about the matter, this can help to bring pressure to bear on the Indonesians. For far too long the Indonesians have been able to perpetuate appalling violations of human rights because of the widespread ignorance in the Western world about what is happening. This is a situation that must not be allowed to continue.

Communications: Exchange on the Philippines

Madison, Wisconsin

Dear Sirs:

In the December 1973 issue you printed an article by Ms. Luzviminda Francisco[1] in which she attempted to sketch in the "nature of America's policy of aggression, the depth of popular mass resistance to the American forces and the duration of the struggle. . . ." (p. 3) in order to move against "Filipino false consciousness" (p. 2) of the American connection in the history of the Philippines. It is a brief survey and rather well done, although one may quibble about the relationship of excesses (what a tame word compared to the record she presents!) of war and the nature of imperialism as well as implying that there was a "nation," a "Philippine society," or a "national struggle" (p. 3) before the late 19th century, at the earliest.

I am concerned however about some errors of fact and questions of interpretation deriving from her comments about the "Balangiga massacre" and General "Howlin' Jake" Smith's words and actions in response. On page 8 it was said

> *In late September [1901], in the town of Balangiga, Samar, American troops had for some time been abusing the townspeople by packing them into open wooden pens at night where they were forced to sleep standing in the rain. Several score of guerrilla Gen. Vicente Lukban's bolomen infiltrated the town and on the morning of September 28, while the Americans were eating their breakfast, Lukban's men suddenly fell upon them. Heads dropped . . . Fifty-four Americans were boloed to death, and few of the eighteen survivors escaped serious injury.*[32]
>
> *The Balangiga massacre initiated a reign of terror the likes of which had not yet been seen in this war. . . .*

The reference number 32 she cited was to "Testimony of William J. Gibbs, a survivor of the massacre, Senate Doc. no. 331, part 3, 2284 et seq.". I have not seen this source but another source, also cited by Ms. Francisco, quotes extensively from the same document and survivor.[2] According to Gibbs, Captain Connell tried to muster the men of the town for forced labor and, faced with the passive resistance of the townspeople who did not show up for work, he rounded the men up, forced them to work under armed guard, and at night

> *"they put the ninety natives in the two Sibley tents, which only held about sixteen soldiers, they could not lie down; they had to stand up. There was not room enough to lie down. They stayed there for two or three days. . . ."*

working during the days. After four days they were allowed to go home but forced to continue this work in the day time. Seventy-five more men were brought in by "the native chief of police . . . from the mountains, who were confined for a week in the Sibley tents. . . ." Captain Connell also destroyed food stocks. His forced labor extractions lasted from July until the "massacre" on the 28th of September.

Contrary to what Ms. Francisco said, Sibley tents are not stockades and the forced labor was not kept standing in the rain, albeit standing in a close, very humid and uncomfortable environment.

The "Historical Data Papers" in the Philippine National Library, compiled by schoolteachers in the 1950s from sources in the towns in which they taught,[3] amplify the complaints of the townspeople against the Americans. The Balangiga section emphasizes the resentment caused by imprisonment on slight offenses and insults of the women by Americans. The account, written with a great deal of pride, tells in detail how the "massacre" was planned and organized by Valeriano Abanador, how he brought in the extra men (Lukban's?), how he signalled the attack by whirling his cane in the town plaza, etc. Later he and his followers and the townspeople fled and the Americans came back and burned the town. In April of 1902 he surrendered through the good offices of his brother, Pedro, and accepted the general amnesty offered by the Americans.

These papers on Samar are interesting too on the questions they raise about the impact of the war on the Samareños. Ms. Francisco quotes Gen. Smith and his famous threats to turn Samar into a "howling wilderness' ", his orders to " 'Kill and burn . . .' ", to kill " 'everything over ten' " (p. 8). One contemporary defender of these orders and threats argued that

> *Translated [!] into less dramatic language, they simply meant, "Attack the enemy wherever you see him, and destroy all his supplies and quarters." . . . So, too, with that order to kill every male over the age of ten years, which meant every male encountered under arms and no more. General Smith was not waging war against women and children. In short the campaign in Samar was simply a war of exceptional severity waged against an enemy of exceptionally savage and treacherous nature.*[4]

This is clearly special pleading and his descriptions of the Samareños can be dismissed out of hand. Nonetheless, it is striking that in the seven volumes of "Historical Data Papers" on Samar there are no accounts of "My Lai"-type atrocities. There are many burnings of villages and towns (by Americans, revolutionaries, and *pulahanes*) recounted, and there are

1. Luzviminda Francisco, "The First Vietnam: The Philippine-American War of 1899," *Bulletin of Concerned Asian Scholars,* 5:4 (Dec. 1973), p. 2-16.

2. Moorfield Storey and Julian Codman. *Secretary Root's Record. "Marked Severities" in Philippine Warfare.* Boston: Geo. H. Ellis Co., 1902, p. 30-32. Storey and Codman, by the way, say that all but 29 men were killed from the detachment, twenty-two of which were wounded (p. 29-30). The "Historical Data Papers" (see below) say 57 Americans, officers and men, were killed (v. 1 of Samar volumes, Samar-in-general section).

3. See my "The Historical Data Papers as a Source of Filipiniana," *Bulletin* of the American Historical Collection (Manila), 1:2 (March 1973), pp. 14-23, for more information on this source.

4. Storey and Codman, *op. cit.,* p. 116, citing a letter by Henry Loomis Nelson to the Boston *Herald* of August 25, 1902.

examples of being forced to live in concentrations, etc., but there are no accounts of armed sorties against civilians or genocidal actions.

This absence may be attributable to what Ms. Francisco terms "the traumatizing effect of mystification and false consciousness regarding the American colonial period" as well as to the well-known observation "that the winners get to write the history books" (p. 2). On the other hand, such a conclusion is not necessarily accurate regarding the American impact on Samar. At the very least the question should be seen as an open one, especially when one encounters the proud account by the Balangiga people of their ancestors' remarkable attack (slaughter really) on unarmed and unsuspecting American troops with no accusations of massacres of civilians made against those Americans. A careful study of the Revolution (and subsequent *pulahan* activities) on Samar is clearly overdue, hopefully by someone with a master of English, Spanish, and Waray who is able to spend considerable time on Samar.

Robert B. Cruikshank

London

Dear *Bulletin:*

Thank you for forwarding Mr. Cruikshank's recent letter. Leaving aside questions of less-than-earthshattering moment such as whether Balangiga townspeople were kept in tents or stockades, and whether fifty-four or fifty-seven Americans were killed in the massacre there, it seems to me that he raises an important question by asking what precisely did happen in Samar? The evidence is, admittedly, fragmentary. What is known, not only about the Samar campaign but about American conduct of the war in general, is the length to which the War Department was prepared to go in concealing the true nature of the struggle in the Philippines and in minimizing and belittling charges of atrocity when they could not be denied. Indeed, this is a point I tried to emphasize in the article.

The problem for the War Department was that it could not get the generals to keep their mouths shut. Thus General Smith's orders to "kill everything over ten" and to turn Samar into a "howling wilderness." Did he actually accomplish his mission? In the literal sense undoubtedly not. But given the man's blood lust and his previous record in the Philippines – and in the American Indian wars before that – I do not think Smith can be faulted for lack of effort. We are left to speculate on just what Smith was able to accomplish in Samar, how much or how little of the island was actually turned into a howling wilderness, and, presumably, how many eleven-year-old lads escaped his noose. Should the Americans then be applauded for failing to carry out the full measure of Smith's apocalyptic vision? Vietnam wasn't bombed back to the stone age either – only nearly.

Mr. Cruikshank's commendable concern for accuracy of detail makes his willingness to acquit Americans of misconduct in Samar on the basis of material contained within the Historical Data Papers – compiled and written fully fifty years after the event – all the more puzzling. It is unfortunate, too, that Mr. Cruikshank does not intend to extend his own study past 1898, for if he did he might find out for himself what those "unsuspecting" Americans were up to when they got their heads chopped off.

Luzviminda Francisco

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THE *GOUFFRE À BATAILLONS* – A French Artillery Officer at Dienbienphu, 1954

BY JOHN COMER

If I wake up in the morning with an ache in my left ball I send a couple of rounds through the post just to let them know I'm off for breakfast.

That usually gets rid of it.

It's a philosophy of mine – by lunchtime I'm immune to anything and you can't explain that entirely in terms of bacon and eggs.

I have never once had the slightest ache in my right ball and I don't intend to start now because a few poppy merchants have got the wind up or they can't shift rubber like they did in the days of yore, and I've seen enough fields and continents to recognise symptoms even if some of you don't know what it's for between your legs.

If I don't wake up at all my batman phones the mortuary officer to get off his arse and send out a Dakota. Nobody wants to spend eternity in a pile of dog shit let alone in the provinces.

Let me describe the enemy for you.

I might as well invent a picture of air as of anything else. I can't walk through an empty village without noticing recently-scuffed earth.

The next day it's sprouting herbacious borders like any rustic retreat, a very scenic amalgamation of shrubs. It's a mirage, it's all air when you touch it.

It takes one woman to pull a pin
and everybody's kicking up like turtles.

And who's going to listen whether you're professing your mothers or the Mother of God, leaning on virgins for succour, or one-time virgins for all that that matters – it's only a platoon sucked into a paddy field, another handful of Locals who couldn't keep their eyes open along the co-ordinates, and not a ripple to show for it.

But that's in the past and I won't linger on it in the present circumstances, which is a precise attrition indeed and entirely European.

I won't extrapolate to a disaster
simply from ten years of inadequate engineering.

There's nothing for me in a bunch of clapped-out Expeditionaries who can't contain a text-book fortification against a consortium of desperadoes pushing Peugeots along antique gulfs and goat walks where you have to watch your shoulder blades getting nipped by the jungle growing behind you

even if they can gut a small hill
overnight with bare hands, a hundred of them together with a dozen picks and stuffed with ideas.

I've got my eye on a spot of chain-harrowing if the worst does come to the worst, if I don't start up a cement factory: getting out into the open, the broad fields again, and the corn coming off the harvesters in those big hessian bags, and sticking my arms into it to feel the torrent of grain.

That's my sentimental side
and I can wallow in it like anyone else.

I sometimes wonder myself what we're doing right up here in this place. In this hole on the border, almost in Laos. This man-eating hole, this meat-grinder

– grinding meat, gorging batallions –

whether we're here swilling *ordinaire* so as the Chateau Lafite can tickle a few palates with uncontaminated grapes. And a fair amount they have to pay for that if they're not fobbed off with bourgeois growths, and us with the last run of leached-out skins and stems and whatever else they put in it apart from sulphur.

– In six months we'll be getting well and truly pissed on it ourselves in Dalat, all you great eaters of frogs, you men from Sidi bel Abbes.

We'll even put on some knackwurst for our other contingent, and a jar of acid to wash it down with that they seem to like, though I don't call it anything that ever grew on a vine.

There'll be more than a finger by the time we're finished if I've got any grasp of things.

These women now they'd as soon swallow your balls as give your hardware an inch: they'll be dancing to the Marseillaise as though they were born to it. I don't need to take you on a tour of the vineyards for you to see that.

In the meanwhile, this is where we are situated and it's not just any boulevard in the world whatever reasons you think up.

It's not just any common bank of daffs and primroses with little Marianne drops-her-knickers from the bakery stretched out and your tongue lapping the ermine.

It's more like bunking up in a broom closet if you've ever tried that in a hail of wax tins and rugby boots, and a coal shovel up your arse for good measure

– this rat-run, this grinder of battalions –

But that's the price of a uterus, and it brings the best out of some women, women of appetite, women going hell for leather like these are.

It can't last much longer by any account,
and we'll all be sweeter in time
once we see who is left standing.

Forays into Japanese Cultural Psychology

by Don Roden

Socialization for Achievement: Essays on the Cultural Psychology of the Japanese, by George De Vos. Berkeley: University of California Press, 1973. 500 pages. $20.

I.

The search for a theory of Japanese personality and culture has been an epic endeavor among foreign specialists, filled with all the mystery and fantasy of discovering Atlantis. The inquiry began nearly a century ago when the venerable William Griffis described the moral character of the "average Japanese" as being "frank, honest, kind, gentle, courteous, confiding, affectionate, filial, loyal." Reviewing his own inventory of personality traits, Griffis felt obliged to conclude that the "Japanese are simply human, no better, no worse than mankind outside."[1]

Unfortunately, Griffis' warning against reducing the Japanese to either "guileless innocents" or "sinners above all nations" was not heeded by many of his fellow travelers. For example, General Horace Capron, in a fit of characteristic pique, attached a "semi-barbarian" label to Japan, protesting that everything about the country was mysterious and hence beyond the limits of "Anglo-Saxon" comprehension.[2]

Other, more scholarly visitors, like Lafcadio Hearn and Charles Thwing, found the "mysteries of the East" to be more a source of intellectual intrigue or fascination rather than simple-minded exasperation. Hearn, in particular, popularized the image of a Japanese "Fairyland" – "... a world that is not, and never could be your own," but which is as exciting to explore as ancient Egypt or Nineveh.[3] When explored by Lafcadio Hearn, the "hidden soul" of the Japanese takes on a certain eloquent if sometimes eerie quality; but few redeeming words can be spared on the ordinary travel and description writers who profited handsomely from their portrayals of "rare days in Japan" and "journeys among the gentle Japs."[4]

The extent to which this Gilbert and Sullivan approach to Japanese custom and life-style contributed to international distrust is difficult to determine. Still, few can deny that many of the books pretending to "unmask Japan" merely furnished stereotypes to be worked over and embellished by the comic-strip hacks and cartoonists of the prewar period.

Ironically (or should I say predictably), the actual outbreak of hostilities between the U.S. and Japan did more than any previous event to inject new life into the study of Japanese personality and culture. While the popular press continued its relentless bombardment of the public with images of buck teeth and oily smiles, research programs were being set up in Washington for more serious inquiries into Japanese national character. However, this sudden swelling of scholarly interest in Japanese personality and culture was by no means a neutral academic development. Psychological warfare – "sykewar" – had captured the imagination of the Office of War Information; and when the trumpets sounded for the nation's meager band of cultural anthropologists to abandon their halls of ivy for "patriotic research" in Washington, there was no question that the government was primarily interested in deciphering "modal personalities" for "enemy" nations[5] – above all, Japan.

The research fruits of this campaign sponsored by the Foreign Morale Analysis Division of the Office of War Information are now legion. Heading the list of entries is Weston La Barre's famous "observations"[6] wherein Japanese character is reduced to compulsive neurosis and a tyrannical superego. In a somewhat similar vein, Geoffrey Gorer[7] identified harsh discipline in the vicinity of the toilet as being the bane of healthy character development in Japan. And

finally, there is Ruth Benedict's *The Chrysanthemum and the Sword,*[8] undoubtedly the best of the wartime studies, although she is often criticized for overplaying the "unique" features of Japanese personality and for using a framework which is both static and ahistorical. The book did receive considerable attention in Japan, however, and provided an important stimulus for the postwar development of the social sciences in Japanese universities.

Since the publication of Benedict's magnum opus, the major studies on Japanese personality and culture have been written in Japan, beginning with the works of such social scientists as Minami Hiroshi, Kawashima Takeyoshi, Wagatsuma Hiroshi and Kamishima Jirō.[9] Two recent books which have probably created the greatest stir in academic circles on both sides of the Pacific are Doi Takeo's *Anatomy of Dependence* and Nakane Chie's *Japanese Society.* Now available in English translation, the two books are deserving of brief criticism.

Professor Doi, a psychiatrist from Tokyo University, has gained notoriety for his analysis of *amae* (dependent love) as the key mechanism operating in Japanese personal relations. He argues that the web of human relationships beginning with the mother and her son and extending to pseudo-kinship ties between employer and employee, teacher and student, and even emperor and subject, is underscored by the desire of the subordinate party to produce a state of being loved or being cared for by the superior party. The reason being that the Japanese, in Doi's opinion, must "depend and presume on another's love" in order to function effectively in society. Nakane Chie, as a social anthropologist, looks at Japanese society from a different perspective. She is less interested in the quality of human relations between individuals and more interested in the over-all structure of the society. In brief, she argues that Japanese society is characterized by group membership and that the internal structure of the group is further characterized by a vertical hierarchy.

Despite these differences in approach, both Doi and Nakane are implicitly faithful to the cultural relativist position staked out by Benedict and the other early anthropologists. It is not that Doi rejects the possibility of "passive love" influencing human relations in the West,[10] nor that Nakane rules out the "vertical principle" as a tool for cross-cultural comparison. But the burden of their arguments is certainly directed in favor of Japan as *sui generis* rather than Japan as historically comparable with other countries around the world. Neither writer, for example, sees general theory — whether Marxist, Freudian, or Parsonsian — as being broad enough to encompass the peculiarities of "Japanese man." This particularist approach helps to explain why the books have garnered such a wide audience in Japan, where the appetite for self-exploration and self-criticism has been insatiable since the war.

However, while Japanese culture and personality studies continue to fascinate readers in Tokyo, the demand for such studies in the United States diminished noticeably with the end of the American occupation. Once a major impetus for the expansion of Japanese studies in American universities, cultural anthropology was soon swept from center stage of the field to be replaced by the other social sciences — political science, economics, history and sociology. The reasoning behind this abrupt changing of the academic guard can only be surmised. First, the wartime associations with national character studies, developed by anthropologists who had no direct contact with the country they were investigating, probably did not sit well with latter-day "specialists." An even more important reason was the shift in academic and governmental interest from probing the "modal personality" of an "enemy" nation to explaining and justifying the resurgence of Japan as an economic power. The ethnographic bias of anthropology is simply ill-suited for the quantitative demands of developmental theory. Modernization, after all, must be measured by GNP, literacy rates, per-capita incomes and the uses of inanimate energy, *not* the modalities of toilet training.

Yet during those cold wintry days when the market for books on Japanese economic take-off and political development was scoring new highs, Professor George De Vos was one American scholar determined to persevere relentlessly in the footsteps of Ruth Benedict. To be sure, there have been other postwar American anthropologists with a major interest in Japan, but none seem to have been as successful as Professor De Vos in collaborating with noted Japanese colleagues and in broadening the parameters of field research.

Still, it is with mixed emotions that one greets this newly published compendium of his lifework, *Socialization for Achievement.* On one side, the essays, some of which are reprints from days long past, seem faintly anachronistic. Old questions about guilt and shame, *oyabun* and *kobun,* and obligations versus feelings are rehashed or revised, but never mercifully put to rest. Routine homage is paid to those durable institutions of the Japanese collective personality like "paternalism" and "family authority."

The book cannot, however, be brusquely written off as old hat. In contrast to the wartime anthropologists, De Vos is acutely interested in linking child-rearing practices (more importantly, the values that are derived from those practices) to Japan's economic growth and progress. There is also a psychological dimension to De Vos's arguments which may prick the curiosity of a contemporary audience raised on the identity theories of Erikson and the Freudian variations of Marcuse and Fromm. This integration of psychology with anthropology into a theory of social change for Japan has engaged the author's energies for over two decades; hence he cannot be criticized for jumping on the psycho-historical bandwagon — unlike others, who have just discovered that Kido Kōin's rise to fame in Chōshū was really predicated on a delayed resolution of Oedpial hostility.[10a]

II.

The De Vos thesis begins, predictably, with the mother, who both gives life and structures it by lathering her little boys (more than her little girls) in maternal warmth and kindness.*

*The implications of this argument for women's studies are far-reaching and should not be ignored. De Vos is something of a pioneer in his research on Japanese women; but his concepts of maternal suffering and induced dependency threaten to consign the Japanese mother to a permanent state of self-repression. An article which critically appraises the work of De Vos and Doi from the perspective of the women's movement in Japanese social history is badly needed.

As the author sees it, the Japanese mother is really a masochist, bearing without a murmur of protest the responsibility for her children's (and her husband's) infantilism. Sitting back quietly in situations which call for discipline, submitting eagerly to all kinds of capricious demands, the much-abused mother appears to be victimized by the members of her own family — but not completely! Drawing upon Benedict's interpretation of reciprocal obligation and amalgamating Doi's concept of *amae,* Professor De Vos suggests that "the quiet suffering" of the Japanese woman has the unwitting effect of inducing strong feelings of dependence and ultimately guilt among her offspring.[11] During the long years of self-sacrifice when her children are still of pre-school age, the mother is, in effect, building up a solid foundation in I.O.U.s which some day her child is going to feel compelled to repay. How he or she repays this debt is really the crux of De Vos' thesis.

As the title suggests, *Socialization for Achievement* is not a book on Japanese motherhood but is rather a study of the psychology behind the drive for self-improvement in Japan. Child-rearing is important to the author because it represents the first stage in a process of early socialization for an achievement drive that will dominate youth and adulthood. That is, having been overindulged in maternal kindness, the guilt-ridden child soon learns that the one way to compensate for his mother's sacrifices is to meet the community's standards for success. By fulfilling social expectations, he can bring honor to himself and begin to alleviate some of the stored-up feelings of guilt toward his mother. On the other hand, failure to meet the standards for success has the reverse effect of further hurting the mother and reinforcing the pangs of guilt.

Not surprisingly, one central arena for this extraordinary interplay between the need to achieve and the displacement of guilt is the education system. In no way can the child or teenager better display his own diligence and determination to get ahead than by passing the battery of entrance exams that begin with primary school and extend to the top universities. Nothing can make the Japanese "education mama"* happier than to see her son walk proudly down the halls of Tokyo University, though not if he's wearing a helmet and brandishing a stave. The importance De Vos attaches to education in the drive for achievement is symbolically underscored by the selection of a classroom scene to adorn the book jacket. Pictured is a group of uniformed students sitting at their desks with hands folded, eyes fastened, and heads tilting ever so slightly forward as if they were straining to soak up every word from an unseen teacher. Needless to say, there is no fooling around in this determined classroom ("attitudes toward education are intense; guilt over failure also can be intense"[12]); indeed, there is no space for fooling around anywhere in De Vos' humorless paradigm, except at the unsettling risk of being labeled "socially deviant."

While success in school is a good first step toward alleviating childhood guilt, the total gratification of achievement need is a long-term project that obliges the individual to continue striving throughout his or her adult career. According to De Vos, the reason for the durability of this motivation is that the family bonds which first generate the achievement drive are later reinforced by "pseudo-kinship" ties between employer and employed, the ever-familiar *oyabun-kobun* nexus. This bureaucratic paternalism, also "peculiarly Japanese,"[13] takes up any slack that may arise from softening bonds between mother and son, thereby revitalizing the desire to achieve.†

However, the implications of De Vos' "achievement syndrome" extend far beyond the realm of individual biography or case study. Taking extraordinary liberties with social location and historical space, De Vos links his thesis on achievement and family (or pseudo-family) relations to Robert Bellah's concept of a "Japanese Puritan ethic."[14] Diligence, frugality, perseverance and all the other values that encourage hard work and discourage consumption are explained to be rooted in the need for "self-actualization" and the displacement of childhood guilt. Like the Puritans of Europe, De Vos' "Oriental Calvinists" (p. 9) are driven to diligent toil by psychological needs, but their ultimate goal is the alleviation of "a specifically Japanese pattern of guilt," rather than aspirations for heavenly salvation.[15]

Accepting this logic, Japanese modernization boils down to a question of exploiting the "irrational" personality structure of the family (indebtedness, achievement need, willingness to sacrifice) to meet the rational goals of the industrial state. Presumably, the genius of the Meiji leaders was their ability to use the "familial structure" to provide "the emotional and motivational forces . . . that helped maintain and direct Japanese society successfully in the new direction of industrialization."[16] In making this unoriginal prognosis, Professor De Vos explicitly reprimands both classical economists and Marxist theoreticians for failing to come to grips with the unconscious personality drives which he believes are crucial ingredients to post-Meiji social change and development.

III.

De Vos' self-acclaimed aversion for socioeconomic theory does not conceal the fact that he is using some fairly hefty and at times tendentious terminology of his own. Certainly the allusion to "achievement orientation" within the "social ambience we call modernization" is hardly a neutral evaluation. Like so many students of Japanese history and society in the decade now past, Professor De Vos

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*For those readers who might get the uneasy impression from this book that the "education mama" is peculiarly Japanese, a few words from Charles Schulz might be reassuring. From a letter that Linus' mother has slipped into her son's lunch-bag: "Dear Son, Are you studying hard today? Have you made the most of the morning hours? Your father and I want only the best for you, but you must do your part, too . . . Don't fail us . . . Be diligent . . . Lovingly, Mom." In another letter, Linus' mother writes: "Dear Son, This is to wish you well in your studies today . . . Your father and I love you very much . . . We are working and sacrificing so that you might have this education . . . Study hard . . . Make us proud . . . Lovingly, Mom."

Upon reading this letter out loud, Linus breaks out crying, Charlie Brown exclaims "Good grief!" and we suddenly remember that the syndrome of reciprocal obligations is as American as cheesecake and chicken soup. (Charles M. Schulz, *Nobody's Perfect Charlie Brown — Selected Cartoons from You Can Do It Charlie Brown,* Vol. 1, 1962) Thanks to John Dower for discovering these passages.

†What ever happened to simple self-centeredness or personal ambition as possible motivations for getting ahead in Japanese society? This question is squarely addressed by Earl Kinmonth in a thesis on popular intellectual history in the Meiji which promises to demythologize the *oyabun-kobun* nexus.

approaches the problem of achievement with a special reverence, as if the promise of upward mobility were the elixir of civilized life. Depending upon one's definition of modernization, however, there is some question whether achievement is quite the "dynamic force"[17] it is cracked up to be.

To begin with, studies which celebrate achievement tend to ignore ascription,* or at least they put ascription and achievement in separate packages. As one specialist has explained in a characteristic sampling of sociologese: "the quality of the objects [i.e., ascription] in the adaptive situation has no relevance, it is only their performance [i.e., achievement] which counts."[18] Maybe this holds true for automobiles, but in human society performance is not all that counts, even in the "adaptive situation." For by isolating achievement from ascription, the student is, in effect, diminishing the importance of family background and differing levels of wealth in what is an uneven struggle to get ahead in the world.

American social scientists – especially those few remaining loyalists to Parsonsian "grand theory" – have long been criticized for assuming a universal pattern of achievement in modern societies without clarifying who is actually "achieving" and to what extent their "achievement" changes the overall structure of society. In the case of Japan, as De Vos correctly points out, the success ethic is adapted to social positions in society, with each stratum having its own community-recognized goals. Gangsters have one set of goals, village farmers have another, and small entrepreneurs have still another. Hence, most of the occupational mobility which De Vos talks about straddles relatively short distances in social space, like the village farmer in Niike whose "achievement" is circumscribed by his decision never to leave his home, or the independent craftsman of a lower-class Tokyo ward whose years of diligent work enable him, at long last, to set up a small "house factory." But do such minuscule shifts within the manual labor force warrant the grandiose invocation of "socialization for achievement"? If they do, then the adaptation of "achievement motivation" to individually isolated social strata is inherently ideological, the overriding emphasis being given to intra-class rather than inter-class mobility.

My own suspicion is that the achievement opportunities dispensed in Japanese society have done more to reinforce ascriptive locations than to break them down. The paltry rewards given to a tannery worker for thirty years of "service" may advance his relative status to the extent of becoming a factory foreman, but that same reward also serves to legitimize the opportunities for "achievement" that were given to him in the first place. Among white-collar as well as blue-collar workers, "achievement" often goes hand in hand with self-constriction and obsequiousness to authority. Even within the privileged circles of the corporate and governmental elite, individuals must curb their highest ambitions in compliance with the demands of seniority.

As for the education system, which De Vos cites as "the most intense arena for competition observable within Japanese culture today,"[19] statistics indicate that personal successes are usually registered within the range of opportunities afforded by family background. The Ministry of Education's own white paper of 1962 admits that the relative chances of lower-class Japanese students advancing to the universities are less promising than the chances for comparable children in the school systems of the United States, England and the Soviet Union.[20] What must be remembered when reviewing such findings is that the maintenance of social structure in Japan hinges more on the adaptation of achievement motivations to ascribed standards (status, age, sex) than on the free play of real merit. The extraordinary extent, therefore, to which achievement can be tantamount to an irrational repression of self (as opposed to a rational quest for growth and progress) is certainly a question deserving more serious consideration.

A related problem I had with this book stems from Professor De Vos' implicit insistence upon a functional relationship between achievement and social change. Having rejected conflict analysis as inappropriate for Japan, he goes on to treat achievement as the pattern variable which gracefully adjusts to the changing goals of the social system. In fact, so gracefully did Japanese "achievement motivation" adjust to the revised goals of the Meiji state that modernization took place with "relative ease."[21] The "easy transition" is plausible to the author because pre-modern values and customs were "carried over into Japanese industrialization" without causing disruption to the social fabric of society.[22]

Perhaps, but one senses a curious utopianism running through this harmonic blend of history, cultural values and social structure. Japan seems to be characterized less by dynamic change than by a Parsonsian "moving equilibrium" made possible by the adaptive role of achievement. This does not mean that Professor De Vos has sworn total allegiance to "consensus" theoreticians on achievement. Indeed, he criticizes David McClelland *(The Achieving Society)* for suggesting that achievement motivation is universally rooted in individualism, an assertion which is obviously incompatible with his own perceptions about Japan.[23] But disagreements over its psychocultural origins do not conceal the fact that achievement, whether derived from familial dependence or from simple egocentrism, produces the same static quality in De Vos' historical view of Japan that vitiates the conclusions of many historians and sociologists who lean on the bland vocabulary of systems analysis.

What makes this framework especially worrisome is that by stressing the integrative function of achievement in easing social change, De Vos tends to neglect the more disruptive forces in Japanese history. He overlooks, for example, the millennial hysteria of the Bakumatsu period, the peasant revolts of the 1870s and the political violence of the radical phase of the People's Rights Movement. Nor does he allow theoretical space for documenting the seamy side of industrialization in Japan: the urban slums, rampant tuberculosis, rat-infested "poor houses," prematurely aging children[24] . . . the very conditions that Charles Dickens described in mid-Victorian England. He also neglects to mention that industrialization in Japan literally "took off" on the backs of a young and impoverished female labor force contracted to work in the great textile mills at subsistence wages. And finally, there is the ultimate pathology of armed aggression, the ruthless subjugation of continental neighbors which some have interpreted as a transference of the

*"Ascription" here refers to the allocation of occupational roles on the basis of heredity, sex and/or age, as opposed to individual performance or the demonstration of merit.

oppression which already existed at home.

Of course, in assuming "the ease of Japanese modernization," Professor De Vos is reflecting a point of view that was shared by a number of American historians and social scientists a few years ago. But today most would agree that if a theory of social change is to be convincingly applied to recent Japanese history, it must come to grips with the human costs of modernization throughout the empire. Facile generalizations about the adaptability of achievement values for economic growth simply do not tell the whole story.

IV.

Despite "the ease of Japanese modernization," Professor De Vos is sensitive to the fact that some persons have fallen by the wayside. Writers, university students, juvenile delinquents, gangsters and Burakumin outcastes constitute the hodge-podge of strata that have at varying times been unable to meet the normative standards for achievement. In the last half of his book, the author explores the special circumstances that give rise to "socially deviant behavior" among each of these groups.

Since mother/son interdependence is seen as a key to normative socialization for achievement, it is not surprising that De Vos should see any disruption of this bond as having causal bearing on delinquency, alienation and even suicide. The children of Burakumin outcasts, for example, are described as not having the warm, soothing parental relationships of middle-class youth,[25] while "delinquency formation" in general is said to grow out of parental rejection.[26] Among middle-class students and intellectuals, a similar "absence of maternal closeness" is interpreted by De Vos as having a causal influence upon alienation, self repulsion and suicide. As an illustration, Akutagawa Ryūosuke's mother is said to have suffered from "postpartum psychosis,"[27] crippling her performance of eliciting *amae* and presumably contributing to her son's neurosis and ultimate self-destruction. Maternal distance also tortured the minds of Dazai Osamu and Mishima Yukio.[28] While not saying so explicitly, Professor De Vos comes disturbingly close to the formulation that maternal suffering is to achievement orientation what maternal neglect is to social deviance. One wonders if there is any middle ground where love of children can co-exist with freedom and independence for the mother.

Besides the impact of "maternal deprivation," De Vos does suggest that some of the same environmental conditions which swell crime rates in the industrial West have had a similar influence on crime rates in Japan: migrations from countryside to city, overcrowded housing, increasing violence on the media. However, he stresses the point that even in urban industrial centers, the Japanese family has been able to survive as a powerful unit of social integration, thereby "inoculating" many would-be delinquents from the potentially disruptive conditions that surround them.[29] Apparently the strong family fiber remains intact even for second- and third-generation Japanese-Americans who do not "succumb to the influence of high delinquency neighborhoods" in Chicago and other U.S. cities.[30] The only exception to this general assessment is the Burakumin outcaste whose family relations are notoriously "more fragile" than those of the rest of the Japanese population. The Burakumin must also bear the additional burden of "psychological lag," the continuation of a pattern of internalized deviance and negative self-identity despite structural reforms granting full equality.[31]

De Vos should be credited for initiating a serious discussion of minority status, juvenile delinquency and crime in Japan. The problem, once again, is his choice of terms and definitions which are too confining. By defining achievement as the normative standard for Japanese society, he gives himself no alternative but to brand the lack of achievement or variant expressions of achievement as socially deviant. There are, moreover, ideological implications to this line of reasoning, since some of the people who deviate from conventional performance norms see those norms as merely symbols of legitimation for an unacceptable structure of political and commercial authority.

For example, referring to the rise of the student left, De Vos remarks that Japan since 1945 has witnessed "noticeable trends toward increased deviancy, especially in violence, and political confrontation among youth."[32] These dramatic outcries of dissatisfaction with existing authority are the "negative sides" to the search for identity. They can be viewed "positively" when examined "in the context of American social values"; but in the context of Japanese cultural psychology, the social criticism and activist demonstrations by youth are "eloquent" yet "disturbing."[33] Where Professor De Vos stands on this important issue of student protest and social deviance is blurred at best. Recent reports of the psychological harassment of a few university professors are indeed disturbing. But De Vos has gone too far in ruling out vigorous dissent as a rational, alternative standard to the self-abnegating demands of the Japanese business state and Coca Cola culture. Moreover, he neglects to emphasize the point that a healthy quest for adolescent identity is most often sought in opposition to, rather than in bland harmony with, the conventional rhythms of society.

Finally, one must question De Vos' apparent restriction of social deviance to minority groups, juvenile delinquents, radical students and gangsters. A fully balanced picture of anti-social behavior requires a serious examination of white collar crime which has never bypassed Japanese political or business circles. Bribery, tax evasion, embezzlement, price fixing, pay-offs, cover-ups . . . the list of possible areas for exploration is seemingly endless, yet conspicuously missing in this book. Viewed from the perspective of Watergate, the omission of white collar crime is glaring.

V

Lest one come to the hasty conclusion that *Socialization for Achievement* is a theoretical bag of sand, it is well to remember that the essays which make up the volume were written over the span of two decades, and that only the thin twine of a few introductory pages is intended to hold the scattered pieces together. Individual essays, moreover, reflect an enormous expenditure of collaborative research effort backed by an impressive, if soporific, arsenal of Rorschach tests, Thematic Apperception tests, nameless case studies, graphs and tables. Furthermore, several of the essays, in particular "Some Observations of Guilt in Relation to Achievement and Arranged Marriage" and "Role Narcissism and the Etiology of Japanese Suicide," are must reading. The former may be Professor De Vos' most important theoretical work, while the latter is a controversial yet interesting integration of Durkheim's societal explanation of suicide with

De Vos' own psychocultural approach. The influence of these and other essays is immediately apparent when reading a volume like *Personality in Japanese History*,[34] where the intellectual debt to De Vos is considerable.

Nevertheless, today's student of Japanese culture and society is justified in expecting much more than she or he gets out of a book which is billed as a "psychological anthropology" of the Japanese. Psychological anthropology, as an interdisciplinary innovation of recent years, is presumed to transcend the old-fashioned anthropological lock step of evaluating an individual culture strictly on its own terms. Yet with distressingly few exceptions, rigorous comparative analysis is not to be found in *Socialization for Achievement.* The Burakumin community is discussed with only the barest reference to American Blacks and with no mention of either the Indian caste system or the traditional absence of caste in China. "Violence and social change" at the universities is discussed with only an oblique suggestion of the world-wide dimensions of campus turmoil in the '60s.

Instead of searching for inter-cultural negotiability, the essays sooner or later tumble into time-worn cliches about "a specifically Japanese pattern of guilt," "a characteristically 'Japanese' way," "the particular personality propensities of the Japanese," "peculiar Japanese modes of achievement," and, oh yes, those "unique features of Japanese psychology."[35] That a number of American and Japanese academics have been similarly prone to see Japan as *sui generis* does not soften my criticism. For the cultural relativist's reduction of personality to national character not only impedes comparative analysis, but it diverts the student's attention from the more important problems of politics, social class, and power – none of which should be off limits to the anthropologist.[36] Rather than forging ahead with a cross-cultural analysis that can deal with the psychology of conflict and social change, Professor De Vos seems inexorably bent on throwing the entire discourse back in the direction of Benedict, La Barre and perhaps even Lafcadio Hearn. A century has passed since William Griffis wisely cautioned not to look upon the Japanese as a unique cultural mutation, how much longer will it take for his message to sink in completely?

Notes:

* My thanks to John Dower, Joe Moore, Frank Baldwin, Jon Livingston, and Felicia Oldfather for reading this review and making a number of helpful suggestions.

1. William E. Griffis, *The Mikado's Empire* (New York: Harper, 1903), pp. 569-70.

2. Taken from a seminar paper on Horace Capron and the Meiji Restoration; portions of Capron's memoirs have been published in *The Journal of the State Historical Society of Illinois*, vol. 18, no. 2.

3. Lafcadio Hearn, *Japan. An Attempt at Interpretation* (New York: MacMillan, 1904), p. 19.

4. Rev. J. LL. Thomas ("Vicar of Aberpergwm"), *Journeys Among the Gentle Japs* (London. Sampson, 1897). George T. Ladd, *Rare Days in Japan* (New York: Dodd, 1910).

5. John J. Honigmann, *Culture and Personality* (New York: Harper, 1954), pp. 73-75.

6. Weston La Barre, "Some Observations on Character Structure in the Orient: The Japanese," *Psychiatry*, 8:319-42; also cited in Honigmann, pp. 59-60.

7. Geoffrey Gorer, "Themes in Japanese Culture," (reprinted in D. G. Haring, ed., *Personal Character and Cultural Milieu*) and cited in Honigmann, pp. 257-59.

8. It is Benedict who constructed the now familiar paradigm wherein the performance of duties (*giri*) is seen as a form of repayment for obligations incurred (*on*). She also stressed the tension between duties and human feelings (*ninjō*).

9. For more on this, see Hiroshi Wagatsuma's, "Major trends in Social Psychology in Japan," *American Behavioral Scientist*, vol. 12 (January-February, 1969).

10. Doi believes that *amae* is similar to what Freud called "the child's primary object choice" or what Michael Balint called "passive object love," but that Western psychologists have attached relatively minor significance to this form of infantile behavior because of the dominance of individualism and "active love." *Anatomy of Dependence* (Tokyo: Kodansha International, 1973), p. 20.

10a. "Kido Kōin and Ōkubo Toshimichi: A Psychohistorical Analysis," by Albert M. Craig, in *Personality and Japanese History*, eds. Albert M. Craig and Donald Shively (Berkeley: Univ. of California Press, 1970). The essays in this volume are representative of the current academic interest in the psychological biographies of Japan's cultural and political leaders.

11. George De Vos, *Socialization for Achievement*, p. 149. For a distillation of De Vos' argument see his article with Edward Norbeck entitled "Culture and Personality: the Japanese," to be found in *Psychological Anthropology*, edited by Francis Hsu (Homewood, Il.: Dorsey Press, 1961).

12. Taken from the caption for the jacket picture, reprinted between pages 270-71 of *Socialization for Achievement.* References to the "education mama" and the "intense" attitude toward education are on pp. 36-37, 181.

13. De Vos, p. 168.

14. Robert Bellah, *Tokugawa Religion* (Boston: Beacon, 1957). The terms "Japanese Puritan ethic" and "Oriental Calvinists" are used by De Vos.

15. De Vos, pp. 9, 106, 147.

16. De Vos, p. 199.

17 De Vos, p. 172. He describes achivement motivation as not being "static" but rather serving as "a dynamic force for social change in society."

18. Bellah, p. 3. In the following discussion of achievement and ascription, I have been influenced by Alvin Gouldner's *The Coming Crisis of Western Sociology* and C. Wright Mills' *The Sociological Imagination.*

19. De Vos, p. 36.

20. See the article by Ronald Dore in *Pacific Affairs* 37:1 (Spring 1964) entitled "Education in Japan's Growth," p. 73.

21. De Vos, p. 207.

22. De Vos, p. 204.

23. He criticizes David McClelland's *The Achieving Society* and Everett Hagen's *On the Theory of Social Change* on the grounds that neither book is able to account for psychocultural variations in Japan. McClelland's book in particular annoys De Vos because it "generalizes a relationship between achievement need and individualism" that does not hold true for Japan. (pp. 167, 170-186) De Vos credits Hagen's attempt to cope with differential modernization within a broad cross-cultural framework (179), but insists that even Hagen fails to grasp "some unique features of Japanese psychology." (195)

24. The costs of industrialization are itemized in Sumiya Mikio's *Social Impact of Industrialization in Japan* (UNESCO, 1963).

25. De Vos, pp. 400-01.

26. De Vos, p. 307.

27. De Vos, pp. 512.

28. De Vos, pp. 481, 524-529.

29. De Vos, p. 368.

30. De Vos, pp. 355-56.

31. De Vos, p. 390.

32. De Vos, p. 254.

33. De Vos, p. 255.

34. Albert M. Craig and Donald Shively, *Personality in Japanese History* (Berkeley: University of California Press, 1970).

35. In order of their appearance in my sentence: De Vos, pp. 147, 481, 482, 207, 195.

36. As Clifford Geertz, for one, has demonstrated in his work on Indonesia. Also Robert Lifton, though not an anthropologist, has nevertheless raised some interesting questions about the psychology of "historical dislocation" in Japan which bear directly upon several of De Vos' topics. Cf. *History and Human Survival* (New York: Random House, 1970). But Lifton, like Geertz, is never mentioned in *Socialization for Achievement.*

The Academy on Japanese Foreign Policy

by Gordon Bennett

Ellingworth, Richard, *Japanese Economic Policies and Security*, Adelphi Paper No. 90 (London: International Institute for Strategic Studies, 1972). 36 pp. $1.00. These papers may be ordered, post-free, from the Institute, 18 Adam Street, London, WC2N 6AL, England.

Emmerson, John K., *Arms, Yen and Power: The Japanese Dilemma* (New York: Dunellen, 1971). 420 pp. $15.00.

Hellmann, Donald C., *Japan and East Asia: The New International Order* (New York: Praeger, 1972). 243 pp. $8.00.

Kōsaka, Masataka, *Options for Japan's Foreign Policy*, Adelphi Paper No. 97 (1973). 40 pp. $1.00.

Langdon, F. C., *Japan's Foreign Policy* (Vancouver: University of British Columbia Press, 1973). 231 pp. $9.00.

Morley, James W., ed., *Forecast for Japan. Security in the 1970s* (Princeton: Princeton University Press, 1972). 249 pp. $9.50.

Muraoka, Kunio, *Japanese Security and the United States*, Adelphi Paper No. 95 (1973). 39 pp. $1.00.

Scalapino, Robert, *American-Japanese Relations in a Changing Era* (New York: Library Press, 1972). 127 pp. $2.50 paper.

Weinstein, Martin E., *Japan's Postwar Defense Policy, 1947-68* (New York: Columbia University Press, 1971). 158 pp. $7.50.

Japan's foreign policy was all but totally ignored by American scholars and politicians alike through the late 1960s. The recent rash of English-language publishing on the subject, therefore, amounting to at least thirteen major monographs since 1970, is itself an occurrence worthy of note.[1] Well-known works on U.S. foreign policy of the Kennedy and Johnson years (Schlesinger's *Thousand Days*, Hilsman's *To Move a Nation*, Walton's *Cold War and Counterrevolution*, LBJ's *Vantage Point*, and others) fail to devote even one single paragraph to the role expected of Japan under America's order in the Pacific. And the first five volumes of this *Bulletin* (1969-73) contain only four articles about current Japanese foreign affairs, three of them by the same author, Herb Bix. Were it not for Bix, John Dower, *Pacific Imperialism Notebook*, and the AMPO Collective in Tokyo, the anti-imperialist wing of American Asian Studies as well would be guilty of a most serious oversight. Now, however, it appears the halycon era of Tokyo's passive free-riding "low posture" is coming to a close. Foreign sectors of both the American and Japanese economies are growing. Competition between the two economies is increasing, especially for limited fuel and raw material resources around the world. The Nixon Doctrine is selectively curtailing the U.S. military presence in Asia. China is expanding its diplomatic capacity in the region, even to the extent of pursuing detente with America. And Tokyo is finally being presented with some real foreign policy choices. The Pacific "Anglo-Saxon lake" may not yet have changed into a multipolar ocean, since the U.S.-Japan alliance remains the defining diplomatic relationship in East Asian international relations. But the whole scene is less predictable now than at any previous time during the postwar Cold War decades. And once again the academic world has responded to emergent international conflict by producing an imposing quantity of policy-oriented writing.

The reason for this flurry of publication is not hard to

discern. Voices from Japan's ruling Liberal Democratic Party (LDP), including that of former Defense Agency Director General Nakasone Yasuhiro, have called openly for a review of the U.S.-Japan Security Treaty in the late 1970s. Moreover, in recent national elections for the Lower House of the Diet (December 1972), left-wing parties antagonistic to the Security Treaty significantly increased their delegate strength. The Japan Communist Party rose from 14 to 39 seats, and the Japan Socialist Party rose from 87 to 118. What's more, the internal distribution of Socialist seats weakened Eda Saburō's right-wing faction vis-a-vis the central party leadership. The LDP itself lost 13 seats (falling from 297 to 284) and thus continued its downward slide. The demise of the LDP into opposition status does not appear on the immediate horizon, but many LDP politicians are beginning to identify a posture of limited loyalty to the Security Treaty as the wave of the future. To foreign policy managers on both sides of the Pacific this trend is dismaying. Officials in the U.S. Department of State, no less than in Japan's *Gaimushō*, regard the Security Treaty as a most valuable diplomatic instrument for controlling forces in their respective domestic politics hostile to the alliance partner across the ocean. Such a treaty probably could not be negotiated today, which in these officials' view is all the more reason for both sides to strive to maintain it. Conflict between Japan and America would be far more severe in its absence.[2]

The nine books and papers under review here display near-consensus in expecting that the U.S.-Japan alliance will continue in the short run, even if modified in some important way. Donald Hellmann stands alone in anticipating a program of heavy military spending and nuclearization in the not-too-distant future. On the narrower but equally interesting question of whether Japan will (or should) move closer to, or farther away from, Washington in the short run, these nine scholars are split down the middle. The half favoring the former view includes Emmerson, Hellmann, Morley, Scalapino and Weinstein (all five Americans). The half favoring the latter view includes Ellingworth, Kōsaka, Langdon, and Muraoka (one Englishman, one Canadian, and two Japanese). Such a breakdown of scholarly prediction by nationality is striking. At a very crude level of analysis, the "American" view seems to be that the Japanese have no place else to go. Not only is this comforting for patriotic citizens to contemplate, it is also extremely reassuring to those American policy-makers who

CHART 1. DISTRIBUTION OF TOPICAL EMPHASIS, BY PUBLICATION

	Ellingworth	Emmerson	Hellmann	Kōsaka	Langdon	Morley	Muraoka	Scalapino	Weinstein
1. General									
A. Explicit analysis of any anti-imperialist critique, in name or in substance	C+	C	C	B–	C	B–	C	C	C
B. Country-by-country analysis	C	A	C	C	C	C	C+	C	C
2. Constraints of capacity									
A. Military	C	A+	C	C+	B–	B	A	B–	A
B. Economic	A	B	B–	A	B–	A	B	A	C
C. Geographical	C	C	C	B–	C	C	C	C+	C
D. Scientific and technological	C+	A	C	C	C	C	A–	C	C
3. Constraints from domestic politics									
A. Who benefits from alternative foreign policies?	C	C+	C	C	C+	B	C	C+	C
B. Domestic sources for expanding Japanese influence abroad	C	C	C	B	C	C	C	B	C
C. Fractionalization of party system; weakening of LDP	C	C	B	B	C+	B+	C	B–	C
D. Popular movements; opposition parties	C	C	C	B	C	C+	C	B	B
4. International constraints									
A. American policy	B–	A	A	B+	A+	C+	B+	A	A
B. Russian policy	A	B+	B–	B	C	C+	B+	B–	B–
C. Chinese policy	A	B+	B–	B	C+	C+	B+	B–	C+
D. Crises in world capitalism	C	C	C	B	C	C	C	C	C
E. Revolution in Asia	C	C	B–	C+	C	C	C	C	C
F. "Multipolarity"	C	C	B	B–	C	B	C+	C+	C

have proceeded from this assumption for years (hence the aforementioned historical neglect).

In their scholarship the nine works as a group are strongest (by both extent of coverage and depth of analysis) on problems related to Japan's military capacity and economic strength, and on problems of Japan's bilateral relations with the United States, China and Russia. As a group they are weakest on their discussion of anti-imperialist critiques of Japanese foreign policy and the U.S.-Japan security alliance, treatment of geographical realities, analysis of which groups in Japan stand to benefit from alternative foreign policies, discussion of crises in world capitalism, and consideration of effects of future revolution in Asia. The proportional attention each author devotes to various topics of significance is elaborated, book by book, in Chart I. An "A" symbol in the chart means the point is singled out for special emphasis; "B" means it is discussed at some length; and "C" means it is discussed only briefly if indeed mentioned at all.

In order best to evaluate these nine works individually, it will be helpful first, in a systematic fashion, to specify plausible alternative paths open to the Japanese government in the decade ahead. This will also provide a common framework for comparing the dissimilar language and concepts employed by different authors.

Path 1 (Independent Power). Pursue "autonomous defense" as a medium-size power, spending at least twice the present GNP proportion (1% or less) on defense. Undertake aggressive, worldwide economic competition with the United States.

1A. (A more extreme variation) Exercise the "nuclear option" through defense spending at the superpower level of up to 10 percent of GNP. Use military forces and armed diplomacy to assure fuel and raw material imports and to secure shipping routes.

Path 2 (Alliance with Washington). Uphold the security treaty with the United States and continue present policies overall.

2A. (One variation) Cooperate more closely with the United States, for example by expanding the Pacific Basin Economic Council from a business association to a regional political entity.

2B. (Another variation) Narrow the range of cooperation with the United States, undertaking instead active economic competition. Increase economic cooperation with the U.S.S.R.

Path 3 (Neutralism). Terminate the Security Treaty and pursue instead unarmed neutrality. Seek multilateral guarantees for Japan's security. Encourage regional development of Southeast Asia with heavier Japanese aid commitments there.

3A. (A variation) Adhere to "neutrality" temporarily, but at the earliest opportunity realign Japan with Asian anti-imperialist governments and movements. Implied in this variation would be a new coalition cabinet in Tokyo composed of Socialists and left-inclined splitaway factions from a weakened LDP.

Two important assumptions which have guided the construction of these alternatives ought to be made explicit. First, *2A* leads most naturally to *1*. That is, the United States for years has urged Japan to accept a greater "share of the burden" for Pacific regional "security"; carried to its logical extreme, however, I believe ironically this shift would lead Japan to an attitude of greater competition with the United States. Second, *2B* leads most naturally to *3*. I believe it is sad but true that continued low defense spending, coupled with termination of the American alliance, would probably make Japan more reactive to American economic initiatives in Asia, and probably also more dependent upon regional organizations in which a leading role would fall to the greater military power in Washington. The longer run is less clear to me, but at the very least I expect a neutral Japan would feel pressured either to re-arm or to re-align.

Since I already have given the nine reviewed works a "grade" of *A*, *B*, or *C* on important topics with respect to the overall emphases of their analyses, I will restrict my separate discussion of each work to a brief review of salient assumptions and arguments. In very rough terms, and I must emphasize this qualification, I have arranged the nine works in declining order of their total interest and usefulness to future researchers. Needless to say, some points are made in pieces toward the end of the list which outshine the analyses of similar points appearing in higher-ranked pieces. I wish to communicate through this ranking nothing more elaborate than the relative strength of general recommendation I can give to each work as a totality.

1. Richard Ellingworth's fact-filled monograph concludes with a well-argued prediction that barring the unforeseen, path *2B* will be trodden. Manifold economic conflicts lie ahead of the United States and Japan, even while "by far the most important element" in Japanese defense policy will remain the American nuclear umbrella. The appeals of path *3*, especially multilateralism and Asian regional development, will continue to outcall the sirens of path *1*: *zaikai* (high financial circles) do not want higher taxes; commercial interests with heavy overseas involvements "doubt whether the development of an enhanced Japanese military capacity would do much to protect their properties"; and a weakened LDP may well be inclined to "emulate the pacifism of the Left" in order to keep its head above water at the polls. "As far as Japanese industry is concerned, the only section likely to favour a rapid military build-up would be the one involved in the production of arms, but this does not constitute a pressure group of great importance in present circumstances." This last statement, though true at the moment, may soon be worthy of some skepticism, since already in 1970 Japan's top five defense contractors (together accounting for 56.5 percent of the arms business) were Mitsubishi Heavy Industries, Kawasaki Heavy Industries, Ishikawajima-Harima Heavy Industries, Mitsubishi Electric, and Nippon Electric; these companies are not political pygmies.[3] Ellingworth provides a fine analysis of Tokyo's complex calculus of the costs and benefits of investing in the Siberian resources of the U.S.S.R., and of how this calculus depends closely upon fluctuations in the Middle Eastern crude oil market. He puts forward the intriguing strategic hypothesis that Japan's extreme vulnerability in oil supplies actually might work in favor of a future government electing path *3*, because this easily interdictable dependence reduces other powers' concern over her becoming a hostile

party during a major war. He introduces, but regrettably fails to develop, the additional hypothesis that Tokyo's strategists reap a parcel of deterrent effect and international prestige gains simply by sustaining an air of ambiguity about their nuclear intentions. Ellingworth is a member of the British Diplomatic Service.

2. **Frank Langdon**, Professor of Political Science at the University of British Columbia, organizes his text about the eras of Prime Ministers Ikeda (1960-64) and Sato (1964-72). The result is a very literate and detailed descriptive comparison of these two premiers' views on defense policy, economic policy and stance toward the Asian region. The most interesting general points, however, are barricaded in a concluding chapter on "Japan as a World Power." There Langdon notes that since 1972 China has ended its bitter and vocal opposition to the Security Treaty, and since the Nixon and Tanaka pilgrimages to Peking, its expressed antagonism to American and Japanese imperialism and to Japanese "militarism" as well. With Soviet "social-imperialism" a more immediate concern to the Chinese government nowadays, Langdon suggests the treaty might even promote Japan's good relations with China by raising the value of Japan as a counter to Russia. He also offers evidence (dating from March 1972) that Sato wished to waffle on his earlier affirmation of joint Japanese-American responsibility for the security of Taiwan and Korea, which had been expressed in the 1969 Nixon-Sato Communiqué announcing the agreement of Okinawa reversion. Finally, he calls attention to an important historical element in both sides' respective views of the Security Treaty. For Japan, the original purpose to get U.S. protection and economic assistance became by 1970 one "to moderate American reactions over economic competition and clashes." For the United States, the main purpose throughout has been to maintain a forward presence in Asia and to involve Japanese power in it: "the Far East clauses [referring to Taiwan, Korea and other areas outside Japan] remain for the U.S. the most prized parts of the treaty." On several other issues, unfortunately, Langdon's analysis is less instructive. Whereas he alludes ominously in his conclusion to problems arising from large amounts of newly available Japanese investment capital, he is nearly silent in the main body of the text on this aspect of Japan's overseas economic policy; his detailed evidence is restricted to the narrower world of trade and the balance of international payments from exports and imports alone. On the crucial question of how closely U.S.-Japanese economic cooperation in Asia is related to the closeness of the security tie, he reaches nearly opposite conclusions within fifteen pages of one another. In the first instance the correlation is held to be positive: "Loosening of the military bonds would permit the greater independence in Japan's foreign policy but it will also increase economic competition or rivalry with its advanced Pacific rim neighbors" (p. 190). Weaker alliance is associated with less cooperation. In the second instance it is held to be negative: "Even the faction leaders supporting Tanaka are willing to see a looser defense arrangement with the United States if lessening regional tensions permit it . . ." (p. 205). Weaker alliance is associated with more cooperation. If this seems less a substantive confrontation than a mere case of imprecise language, the same difficulty doggedly crops up elsewhere. Langdon every now and then is disposed to soaring flights of optimism which defy appreciation by a sceptic. Japan's acceptance as a leading world power, for example, "may require th[illegible] has obvious mutual advantages if Japan is encouraged to be less aligned but committed to a policy of cooperation and assistance in solving the problems of countries, rich and poor alike" (p. 6). Or for another example, "it might also be possible to combine some of the desirable features of both the Japanese and Chinese models of development for use in the other countries of the region. . . ." (p. 196). His prediction of the future is subdued, but adds up to an argument for path *2B*, with pressures toward path *3* evident in the factional coalition behind Tanaka.

3. John Emmerson retired from the U.S. Foreign Service in 1968 to become a Senior Research Fellow at the Hoover Institution and a Research Associate at the Center for Strategic and International Studies (Georgetown University, Washington, D.C.). He began his career in 1936 as a Japanese Language Officer, served with Stilwell and with the American Observers Mission in Yenan during the war, and by 1962-67 had risen to become Deputy Chief of Mission in Tokyo. His writing accordingly is liberally seasoned with anecdotes and vignettes vividly recalled by a practical man who has been close to the Japanese, their language, their culture and their thinking on foreign policy for many years. His prediction for the decade ahead is path *2A* initially, followed eventually by path *1*. "In spite of the tension over voluntary quotas, trade barriers, capital liberalization, and the ever sharper competition in each other's markets and in the world at large, economic questions will probably not seriously threaten the alliance during the decade of the 1970's." He does, however, foresee increasing Japanese independence from the United States both in economic and in strategic affairs, and warns that if U.S.-Japanese relations are not handled carefully during the continuing implementation of the Nixon Doctrine, then even path *1A* is a real possibility. Emmerson's treatment of technological and other constraints upon the path of "autonomous defense" and nuclearization is superb and deserves careful attention from future researchers. In particular he brings out clearly that a *rapid* buildup of nuclear arms in Japan would almost certainly presuppose not only a severely threatened public but also the active collaboration of the United States: it would not be an initiative undertaken over American opposition. One significant corollary which spins off from Emmerson's meticulous attention to technological detail is his revelation of vigorous competition between Self-Defense Forces (SDF) and industrial employers for skilled labor needed by both. This is one of several reasons why many big businessmen do not favor more militarization: "In *Asagumo* (Morning Cloud), the daily newspaper edited and published for members of the Self-Defense Forces, most of the advertisements, often as many as thirty or more and equivalent to three full pages, solicit applications for jobs in private industry." Another is his analysis of the oft-seen catch phrase "60 percent *kokusan*" (domestic production), intended to indicate with pride Japan's current state of self-reliance in defense production.

> *Most of this "domestic production" is actually "domestic assembly," with most vital parts being either imported or produced under license from a foreign company. The phrase "60 percent kokusan" can therefore mean that expenditures for 60 percent of acquired defense equipment are paid out in* yen, *thus saving foreign exchange; but the*

know-how is still imported from abroad, with most of the kokusan *constituting merely assembly, like "putting together a model airplane," as one JDA [Japan Defense Agency] official remarked.*

One shortcoming of Emmerson's work as a basis for looking ahead at Japan's future role in Asia is his relative neglect of pressures from the domestic political scene. A critical implicit assumption behind his stress upon arms, technology and the policies of other great powers is that any cabinet in Tokyo, irrespective of the nature of the coalition behind it, would be compelled to respond to these pressures in roughly the same way. Another is that even though he writes in detail about issues raised by the Japanese left, Emmerson does not choose to joust with the left positions. He is reluctant to bring his data to bear in criticism of arguments he obviously opposes, for example, to the effect that Japanese foreign policy frequently might be distorted by the untoward influence of Japan's big export industries and cartels. Rather, opposition Socialists are generally portrayed as simple negativists who oppose the SDF, oppose the Security Treaty, oppose visits by nuclear-powered ships, oppose the Okinawa reversion treaty, and so on. Characteristically, regarding the issue of the northern islands of Kunashiri and Etorofu (occupied by the U.S.S.R., but which the Japanese government has consistently claimed since 1955) the LDP's position is described without comment as proceeding from the legitimacy of historical boundaries, while the nearly identical Socialist and Communist positions are gratuitously presented as "obvious attempts to curry public favor." The new left is virtually ignored.

4. James Morley, Professor of Government and until recently Director of the East Asian Institute at Columbia University, contributes two of his own essays to this useful edited collection of articles by five specialists (Martin Weinstein, Nathaniel Thayer, Frank Langdon, Donald Hellmann, and Kenneth Young). In his concluding "Forecast with Recommendations," where he draws upon the collective wisdom of his contributors to gaze ahead a few years, he forecasts that "economism" (path *2*) will prevail until 1975 with half a chance of surviving until 1980. As the current LDP hold continues to weaken, though, a new coalition should take shape which will prefer "welfarism" (path *2B*), a program with "several percent more of the GNP" assigned to such domestic items as social welfare, education, housing, transportation, environment and culture. Curiously, this new coalition closely resembles the one other writers feel might easily elect path *3*.

By then the current moves for a reorganization of the labor movement may well have succeeded, with the result that the socialist parties that depend on labor may have reconstituted themselves. The moderate JSP factions may have joined with the DSP factions in a new centrist party. It is possible that the Kōmeitō *also may see the wisdom of joining the new coalition, so that, with the defection of one or more dissident factions of the LDP, the new grouping might then come to power. Of course, it is also possible that the LDP will master its problem of appealing to the new urbanites and will itself continue to rule, but this too will mean new leadership.*

Of other alternatives, "unarmed socialism" (paths *3* and *3A*) strikes him as increasingly unreal, and "chauvinism" (paths *1* and *1A*) he feels would only follow abrupt shifts in international circumstances and the public mood.

Morley's other contribution to this volume, a substantive essay on "Economism and Balanced Defense," presents an uncomplicated but rather ingenious estimate (based on an economic statistic called "gross incremental capital-output ratio") of the cost to growth of higher levels of military spending. However, rather than look for military Keynesianism (positive effects on growth from defense spending), Morley assumes contrary to fact that "all new money for an expanded defense share would be taken away from investment." This assumption makes good sense for his argument that Japan can "afford" militarism, but it makes little sense for a researcher intent upon uncovering what higher military spending might temptingly (and specifically) contribute to GNP.[4] Martin Weinstein in his chapter on "Strategic Thought and the U.S.-Japan Alliance" (drawn primarily from his book—see comments below), predicts path *2* will be taken until either a major shift in leadership or a fundamental change in relations with the United States occurs; then path *1A* is the most likely alternative. Nathaniel Thayer, author of a richly informative monograph, *How the Conservatives Rule Japan* (Princeton, 1969), here restricts himself to the uncontentious prediction that the conservatives will continue in power for another five years. Frank Langdon's chapter, "The Attitudes of the Business Community," is far and away the most valuable piece of research on this critical subject among all the pages under review. After a rather detailed look at the arms makers, the arms exporters and the big business federations, he concludes that "the advocates of accelerated rearmament have not yet captured the citadels of the business community." For the next five years at least:

. . . barring any overriding changes in the distribution of world power, the Japanese business community is prepared to support a modest stepping up of the defense effort, including an increase in GNP share to somewhere between 1 and 2 percent a year, a greater emphasis on self-reliance in the production of Japan's arms needs, and a gradual assumption of greater responsibility for the military defense of Japan's own immediate area, looking forward perhaps in the late 1970's, but certainly not before that, to a phasing out of the security arrangements with the United States. But it is not pressing hard for even these modest changes. It is clearly not calling for any massive rearmament. Nor is it prepared to grasp the nuclear nettle, though it is reluctant to foreclose its right to do so by ratifying the Nuclear Non-Proliferation Treaty.

A light moment for me was provided upon encountering a quotation, undoubtedly tongue-in-cheek, from Kikawada Kazutaka, head of the influential *Keizai Dōyūkai* (Japan Committee for Economic Development) and more recently of the even more influential *Sangyō Mondai Kenkyū Kai* (*Sanken*—Industrial Problems Study Council): "I think the international division of labor from now on should be that the United States handle military affairs and Japan economic cooperation."[5] Donald Hellmann (see comments on his book below) gives strong emphasis to a point which several other writers also advance but with less force—policy immobilism among the factionalized conservative leadership to a degree which prevents bold decision-making by an LDP cabinet.

Anticipating overall "stability" in the government for years to come, he is impelled to conclude that any changes in Japanese foreign policy must come from external stimuli; inexplicably, no stimulus on his list is economic in nature. Kenneth Young, a former U.S. Ambassador to Thailand, argues the case that Southeast Asian countries actually welcome Japanese trade and capital up to a point (he mentions 35-40 percent of total exports and imports), even if Japanese entrepreneurs are looking mainly for cheap and skilled labor and weak anti-pollution legislation. Young's argument is marred by the presence of numberless old saws: "threats" from "China, North Vietnam, intraregional hostilities, and internal revolution or breakdown" all may "endanger" the region, and supposedly it falls within the scope of Japan's interest to help guard against them wherever feasible.

5. Muraoka Kunio, a member of the Japanese Diplomatic Service, determines after a careful weighing of various strategic options that path *2B* lies in Japan's near future. He devotes no attention to domestic constraints in this monograph, but his detailed and lucid review of the international economic and security factors involved is the best and most comprehensive available in a brief work. His analysis does not extend to the competing political aspirations of Japanese and American industrial juggernauts.

6. Kōsaka Masataka, Professor at prestigious Kyoto University and a representative of Japan's establishmentarian "new realist" school of international relations analysis,[6] urges path *2B* moving soon toward path *3*. Political requisites for realization of this alternative, however, are spelled out only vaguely in his monograph. Kōsaka merely expresses a strong preference that Japan, "by creating some kind of co-operation in the security field" among the great powers "can act both economically and politically with less risk of being involved in the Sino-Soviet conflict and with a smaller dependence on the United States." The real interst of the monograph lies rather in his accentuation of this last dimension, the psychological element of dependency upon the United States.

> *It is natural for Japan to dream of some miracle formula by which she can become less dependent on the United States, and in my opinion the strength of the Japanese opposition to the Japan-U.S. Security Treaty could be attributed to this psychological attitude. The make-up of this opposition is complicated and difficult to analyse, but it is not hard to identify the two most important elements: pacifism and nationalism.*

In this interpretation, Kōsaka differs sharply from Emmerson, interestingly enough, who tells us that the 1959-60 struggle against revision of the treaty was more anti-Kishi than anti-American.

7. Robert Scalapino is a widely-published Professor of Political Science at UC-Berkeley who has labored indefatigably and effectively in recent years as an organizer of elite liberal forces to head off dangerous "isolationist" sentiments which he has discerned to be mounting among important sectors of the American public. This mission led him to support vocally and unequivocably even LBJ's level of intervention in Indochina, and hence to campaign with vigor against the "neo-isolationist" anti-war movement. With his reputation thus sullied, it might require a herculean turn of mind for anti-war activists to look at his work seriously. But to do so is not without reward. The present book is Number 2 of "The Washington Papers" from Georgetown University's Center for Strategic and International Studies, a series of authoritative appraisals for the informed general public. Scalapino casts this argument in his familiar thematic mold that "isolationism would merely increase the middle and long-range risks for the United States in an age of more, not less, interdependence among nations." And he concludes, on psychological notes which strike a familiar chord with Kōsaka's "new realism," that a logical affinity of American and Japanese interests in the Pacific has deteriorated sharply in recent years due in part to "a high level of emotionalism" in the human responses of policy-makers: "the atmosphere of distrust and suspicion at elitist levels, gradually reflected at the citizenry level, is the single most ominous trend of the last several years." He urges Washington and Tokyo to work to overcome this estrangement and to reduce the obstacles to "modified alliance" (path *2A*). Readers undoubtedly will be most interested in Scalapino's appraisal and recommendation for the political endeavor they represent, a quest on both sides of the Pacific for a level of consensus sufficient to give the U.S.-Japan Security Treaty its greatest chance to survive. Factors which undermine the alliance are hardly substantial, according to Scalapino; both "emotionalism" and an "atmosphere of distrust" presumably are holes in the dike which organized (and costly) efforts at elite-level cross-national dialogue and at widespread public education, especially on the American side, could plug. But such a framework begs some very important questions. What is the source of this recent "emotionalism?" Whose ox is being gored by the alliance now, and whose ox would be gored if the treaty were to be severely modified or even terminated? In short, who is being emotional about what? The choices Scalapino rejects are the "neutralist" position (path *3*, which he equates with path *3A*), the "pan-Asian" position (my paths include no exact equivalent for this since it is really more of a cultural aspiration shared across Japan's political spectrum than a fully articulated strategic option in its own right), and the "Gaullist" position (path *1A*). As indicated by Chart I, he touches more bases in these 127 pages than any other reviewed, but at a cost of lofty generalization and less detail in any single area.

8. Martin Weinstein elaborates the thesis that Japan's general strategic options really have been so few as to be nearly non-existent: in the last analysis, "the choice of the United States as an ally was almost foregone." A memorandum prepared in 1947 by Foreign Minister Ashida Hitoshi, responding to General MacArthur's March 17 call for early peace negotiations, laid down principles whose fiscal and strategic soundness have endured into the 1970s. Excepting only the Hatoyama Cabinet (1954-56), all postwar Japanese governments have held fast to these principles (path *2*), and Weinstein feels future cabinets will continue to do so even though the Hatoyama approach (path *2B*) will grow in attractiveness toward the end of the present decade. His analysis of deliberations among Japanese policy-makers trying to judge between the relative merits of paths *2* and *2B* is informative on many points although extremely narrow. An important issue he does uniquely analyze at some length is whether the SDF has been conceived in part to protect the ruling conservatives against domestic rebellion. One of Ashida's original principles marked suppression of internal riots or disorders as a sphere of sole Japanese responsibility. Today:

The defense planners do not see this internal threat materializing so long as Japan is prosperous. But as the National Defense Council staff paper implies, and as numerous discussions with Japanese of various political persuasions confirmed, it is widely believed by Japanese that their prosperity and stability are extremely fragile. The general strike attempt of 1947 has not been forgotten, and the memory of it was freshened by the French general strike in May, 1968. The defense planners, as well as most economists and businessmen, are convinced that a general strike would cripple the economy in a matter of weeks, and would be likely to undermine the authority of the Government and the parliamentary democracy which it represents.

On the other hand, as Weinstein takes care to note, a prime minister who actually called upon the SDF to enforce political order when basic consensus was lacking would be "as likely to wreck Japan's present political institutions as to serve them." He also states, but fails to argue, that the Japanese government eschews military Keynesianism for the belief that a small defense budget contributes to economic growth. It is doubtful whether this conclusion in its boldest form could stand up to close examination; I strongly suspect now that various powerful economic interests in Japan are beginning to study the contrary proposition. Weinstein is Associate Professor of Political Science at the University of Illinois.

9. **Donald Hellmann's** book is the least useful of the lot. It began as a consultant's study for the Research and Analysis Corporation and shows it. Approving the fundamental premises of the Nixon Doctrine (though differing on particulars), and admiring the *Realpolitik* "European school of international affairs" exemplified in the works of Henry Kissinger, Hellmann analyzes peace in Asia as a function of a "power balance" in which both superpowers, China and Japan must all participate. As "reality" presses upon them, the Japanese will opt for path *1A* unless "peace breaks out in Asia"; but he is apprehensive about this eventuality because Japan lacks the capacity for "flexible response in a competitive and fluid international milieu" and "is not at all likely to be a skilled player." He sees the Japanese up through 1964 as having behaved more like a trading company than like a nation; but since the "uncertainties" of China's first nuclear blast and of LBJ's decision to bomb-contain the Vietnamese revolution, more "realism" predictably has become evident in Tokyo's foreign policy thinking. Hellmann identifies three "old-fashioned" assumptions underlying his analysis, but he fails to defend taking this step, which in my view is another source of crippling weakness in the book, since all three assumptions are highly questionable. (1) Nation-states in the Pacific region will be the main "actors" in the "subsystem," and not the superpowers intervening from outside. (2) Conflict and war are inevitable features of international relations in the region, given the multitude of revolutionary, destabilizing forces (what magic is it that attracts people so magnetically to the meaningless concept of "stability"?) and almost total absence of compensating counterforces. (3) Japan cannot participate fully or for long in regional politics with a military role narrowly restricted to the home islands. Hellmann is Professor of Political Science at the University of Washington, Seattle.

Conclusion

This review of the liberal Academy's writing on Japanese foreign policy reveals an interesting body of well-informed and highly competent analyses of several important issues. The Academy's publication record also reveals a fresh diversity of views, and on some issues a rather wide diversity. However, it still is of absorbing interest to inquire into the unexamined assumptions which form most of these analyses' common point of departure. No one of the authors reviewed here individually can be described with very great sensitivity by any single set of assumptions claimed to apply to all. Indeed, some of the authors clearly did not begin their investigation with one or more of these "typical" liberal assumptions. The list which follows merely contains assumptions made by most of them most of the time, tacitly in most cases. With a minimum number of exceptions, the list is intended as a fair representation of the jumping-off point of liberal scholarship in general on the subject of Japan's international relations in the 1970s and 1980s.

Assumption One: the sword is mightier than the transistor. Influence in international affairs flows most immediately and directly from military capacity. Therefore, all issues of first importance in U.S.-Japan relations basically must be military and strategic in nature, that is, "security" issues (note the emphasis manifest in line 2A of Chart 1). This "security," often discussed rather abstractly in relation to largely unspecified "threats," is conceived widely to be a collective good (one supplied in equal measure to all members of a society, and which cannot be withheld from one without likewise being denied to all). "Security" is something "Japan" has as a whole, in contrast to being a possession solely of discrete groups, factions or interests within the Japanese polity.[7] Just as Japan's economic progress is dependent upon imported technology from the United States, so also are Japan's present "national security" and international prestige (and low defense budget) dependent upon continuing close ties with U.S. power. As a corollary, style in handling Japanese policy by any administration in Washington can affect the closeness of relations between the two countries significantly only within the narrow boundaries of day-to-day matters. Barring cataclysmic changes in the world and/or in Japanese domestic politics, "Japan's" strategic options in the near future are unlikely to broaden beyond a slight modification (toward either more or less cooperation) of "her" present alliance with America.

But why not assume instead that defense is more of a private good (raising the security of some Japanese, while not affecting or even lowering the security of others), even if publicly paid for and supplied? Whose interests and values in the Japanese political spectrum are upheld by current policies of military building and alliance? Who should stand to win and who to lose if various changes were to be adopted? What would they win and lose? What are the *costs* and *risks* as well as the benefits of maintaining a sufficiently large defense establishment to allow Japanese diplomats to throw their weight around at the international conference table?

Assumption Two: things will continue pretty much as they are now. Sharp and reorienting changes in the structure of world politics, changes sufficient to push Japan's foreign policy off of its present path *2* course onto either path *1* or

path 3, are not imminent. Visible "crises" in world capitalism—such as the recent collapse of the Bretton Woods system of international monetary cooperation and of course the recent turbulence and realignment of world energy markets—are not really enduring crises but merely tough problems for solution by active diplomacy and creation of new cooperative institutions. These problems only temporarily give the appearance of "crises" until the market adjusts to the disturbance or until a new order emerges from negotiations by interested powers. Nor is revolution in Asia expected to rise in importance as a factor in the region's politics. While numerous rebel movements are active in Asian countries, some admittedly large enough to require significant expenditure of government resources to combat them (as in the Philippines, Thailand, and Burma), the regimes in Manila, Bangkok and Rangoon probably are strong enough to withstand the challenges. China seems as restrained as ever in giving active support to these movements (in spite of the doctrinal position approved by the Tenth Party Congress in August 1973 that "relaxation is a temporary and superficial phenomenon, and great disorder will continue"). Vietnam seems to be moving either toward a permanently low level of conflict or toward an unsteady settlement. Both aspects of this assumption are manifest in lines 5D and 5E of Chart 1.

But why not assume instead the possibility of significant restructuring of parts of the international community? Until recently, for example, it used to be argued that prices for primary products in international trade tended to fall even while prices for other commodities rose; hence the terms of trade grew more and more favorable for imperialist countries purchasing their primary product inputs from Third World countries. As shown by the figures in Table 1, extracted by Pierre Jalée from the United Nations *World Economic Survey* for 1963, "countries exporting primary products" (Third World plus Australia and New Zealand) received in the early 1960s only 95 monetary units for one ton of exported fruit compared with 100 units they had received in the early 1950s, and so on for most other products.

TABLE I. UNIT EXPORT VALUES FOR COUNTRIES EXPORTING PRIMARY PRODUCTS, 1959-61
(1950-52 = 100)

Fruit	95
Cocoa, coffee, tea	79
Fats and oils	87
Fibers	63
Natural and synthetic rubber	82
Fuels	103
Non-ferrous metals	94

Source: Pierre Jalée, *The Pillage of the Third World*, translated from the French by Mary Klopper (New York: Monthly Review Press, Modern Reader paperback, 1968), 46.

Moreover, as total trade grew, the proportion among the industrial countries themselves grew as well, while the proportion between industrial powers and Third World countries decreased. Even so, the dependence of manufacturing countries upon raw material supplies from the Third World did not decrease (by 1985, the Interior Department estimates the United States will have to import more than half of its total consumption of nine minerals—aluminum, chromium, iron, lead, manganese, nickel, tin, tungsten and zinc). As a by-product of the current energy crisis, it seems likely that mineral exporting countries will now follow the Arabs' lead and attempt to drive harder bargains with the consuming industrial behemoths. And this bargaining shift is occurring as manufacturers' demands for minerals and other primary products known to exist in limited reserves continue to grow exponentially. The Stanford Research Institute reports rather short periods during which consumption of important minerals doubles: 9 years for aluminum and petroleum, 10-15 for iron, 12-15 for copper, 17 for zinc, and 20 for lead.[8] The influence of producing countries should rise, and competition among consuming industrial countries should increase accordingly.

Also not to be overlooked is the dramatic rise of the Pacific Basin. In the words of a prominent "futurologist":

> The recent emergence of the Pacific Basin as a focus of economic activity has occurred so rapidly and so abruptly that its consequences have not yet been fully taken into account by world political leaders. Since the industrial revolution, the most rapidly growing economic region has always been part of the Atlantic community, either Western Europe or North America, but during the sixties this era came to an end as East Asia greatly outdistanced both. . . .
>
> The year 1968 was a turning point of sorts when Asia displaced Europe as the leading overseas market for U.S. farm products, a position Europe had held since the first exports from Jamestown. The United Kingdom, which historically had dominated Australia's trade with the rest of the world, was replaced by the United States as the principal supplier and by Japan as its principal market, thus weaving Australia into the Pacific community and weaning it from its traditional commonwealth status. These events should not be viewed as temporary phenomena, but rather as part of long-term sweeping trends which are reshaping the global economy. . . .
>
> Transpacific economic ties are developing at a rapid pace in the internationalization of production, international trade, tourism and technology transfer. The bilateral flow of technology between the United States and Japan is the second largest in the world. Impressive though the rate of economic integration among the countries bordering on the Pacific has been over the past decade, there are many indications that the rate at which new economic ties are forming among members of the Pacific community will continue at an equally rapid pace during the 1970s.[9]

Finally, if the historical widening of gaps in material welfare between rich countries and poor is not soon reversed, gaps which overlap the geographic cleavage between industrial North and agricultural South, as well as the cultural cleavage between Caucasian Europe and the dark-skinned Third World, then a new basis for revolution in Asia may develop. Relative deprivation, or more precisely a feeling of lagging behind when others are making it, has been widely argued to be a leading qualitative ingredient in the making of a revolutionary

situation (Tocqueville, Marx, James Davies and Ted Gurr all advance this thesis). At a time when colonies were common in the world, colonial revolts and wars of national liberation were suppressed affairs. Once some of them won their political independence, though, the same aspiration soon grew among the others. Likewise, since World War II neo-imperialism in the sense of persistent imbalance in economic relations and continuously unfavorable terms of trade effectively imposed by metropolitan powers upon their former colonies has been common in the world. But now that some of these lopsided relationships are beginning to be righted, the aspiration for a better balance can be expected to spread accordingly. Simply to ignore (that is assume away) the possibility of future revolution in Asia surely is a dubious step to take.

Assumption Three: Japan's foreign policy mostly is predictable from her overall situation in the world. So overwhelming are constraints upon Japanese policy from the international scene (lines 4A, B and C in Chart 1) that studying how foreign policy issues are managed in Japanese domestic politics, including even the impact of the organized big business community, can yield only small increments of additional explanatory power (line 3B). Forces on the left, mainly due to their ideological rigidity and hence to their perpetual isolation from national-level office (however great their influence in municipal and prefectural affairs), play no visible role whatsoever in foreign affairs (line 3D). From such initial assertions, one more important element of this assumption flows easily: Japan is incapable of pursuing a foreign policy which could be accurately described as "imperialist." Even though the nature of imperialism or neo-imperialism is a question which divides folk, still it should not be difficult for most scholars preferring different formulations to agree in general on two basic requisites: (1) *imperialist powers* must use their military and economic capacity to impose their will in areas of opportunity outside their homeland borders; and (2) *imperialist policies* must derive from expansionist desires rooted within a domestic political economy (that is, national defense measures against genuine threats to the commonweal cannot be considered imperialist). It follows logically, then, that to the extent that Japanese foreign policy is regarded basically as reactive to pressures from abroad, and not basically as a product of coalition struggles at home, the second requisite is not fulfilled and critiques of Japan's behavior on anti-imperialist grounds are inappropriate.[10]

But why not assume instead that different sectors of the Japanese elite, not to mention groups within the public at large, are capable of rather different analyses of emergent trends in the international order, of the danger of Japan's opting for certain paths, of the calculus induced among other powers' strategists by certain Japanese initiatives, or of the extent to which their private gain would necessitate public loss? Then at least the intellectual groundwork is laid to ask who stands to benefit and who to lose from different courses, and consequently, what range of tactics the potential gainers adopt.

Assumption Four: conspiratorial interpretations of East Asian international relations are ludicrous, and scholars who hypothesize them are paranoiac. This is especially true if the alleged conspiracy is cross-national involving elite elements (and perhaps others as well) not only from Japan and the United States but also from such Asian "partners" as South Korea, Taiwan and South Vietnam.

But why not assume instead that the *Pentagon Papers* might reflect reality? Why not entertain the possibility that Peter Dale Scott might have a point?[11] Why not take seriously the chance that temptations similar to those which befell the alleged Watergate conspirators could also prove irresistible to the managers of U.S. and Japanese Asian policy? Conspiracy theories may be immune to disproof (and hence to scientific analysis), but this alone is no reason to ignore their implications.

Assumption Five: Japanese businessmen, like all businessmen, favor a peaceful world "climate" conducive to international trade. When the Japanese government finally does begin taxing its populace more energetically and begins spending a greater proportion of the country's GNP on collective goods, the new expenditures are more likely to be social than military.

But why not assume instead that all businessmen are *not* birds of a feather? Some Japanese traders have major interests in China, for example, while others have major interests in Taiwan. Some may feel strongly that all conditions of peace are not equally desirable. And some may even regard warfare as good for business provided it does not cost too much. Certainly this effect can be observed in American politics. Visible opposition to the Indochina War did not emerge from the American business community until early in the first Nixon Administration (1969 and 1970). Important economic shifts which roughly correlate with this new business spirit of antagonism to the war appear vividly in Table 2. With unemployment beginning to rise, with original war-related *demand-pull* inflation succumbing to *cost-push* inflation, and

TABLE 2. SELECTED U.S. ECONOMIC INDICATORS, 1960-70

	Unemployment Rate, All Workers (Percent)	Consumer Price Index, All Items, For City Wage Earners & Clerical Workers (1967=100)	Total Corporate Profits After Taxes ($ Billions)
1960	5.5	88.7	26.7
1961	6.7	89.6	27.2
1962	5.5	90.6	31.2
1963	5.7	91.7	33.1
1964	5.2	92.9	38.4
1965	4.5	94.5	46.5
1966	3.8	97.2	49.9
1967	3.8	100.0	46.6
1968	3.6	104.2	48.2
1969	3.5	109.8	48.5
1970	4.9	116.1	44.4

Source: *Economic Report of the President,* transmitted to the Congress, February 1971 (Washington, D.C.: U.S. Government Printing Office, 1971), 225, 249 and 282.

(perhaps most importantly) with corporate profits standing still, the business community reacted with intense pessimism to the implications of the Tet Offensive in early 1968 and began to lend their active support to proposals to cut back the Pentagon's forward commitment in Asia (the Nixon plan).[12] Furthermore, why not anticipate that the same analysis of military budgets beginning to hold sway in the United States might also soon prove persuasive in Japan? Why accept out of hand that the Japanese have no equivalent for Defense Secretary Schlesinger, who argued before the House Appropriations Committee in February 1974 that somewhere between one and two billion dollars should be spent in fiscal 1975 not only to accomplish military building which the Pentagon feels is necessary in any event, but also to accomplish "an element of economic stimulus?"[13]

The Academy's labors have borne valuable fruit. But several crucial assumptions have given their work an overall liberal character which is badly in need of critical review. Future researchers definitely must be familiar with the Academy's insights. But also, and more importantly, we must not fail to tackle the many important questions which have gone largely unasked.

Notes

1. Some readers may be interested to know of five other recent works on Japanese foreign policy which for one reason or another have been excluded from this review. Gerald Curtis, ed., *Japanese-American Relations in the 1970s* (1970) and Henry Rosovsky, ed., *Discord in the Pacific* (1972) are two works by well-known Japan specialists put out by Columbia Books (Washington, D.C.) for the American Assembly. Albert Axelbank, *Black Star Over Japan Rising Forces of Militarism* (Hill and Wang, 1972) offers documentation for the argument that Japan is proceeding along the path of rearmament faster than most observers are willing to acknowledge. Jon Halliday and Gavan McCormack, *Japanese Imperialism Today* (Monthly Review Press, 1973) was reviewed by T. A. Bisson in Vol. 6, No. 1 of this *Bulletin*. Arthur Stockwin, *The Japanese Socialist Party and Neutralism* (Cambridge University Press, 1968) is a slightly older monograph, but still useful for historical points.

Other works of lesser importance overall, though still interesting for individual points, include James E. Auer, *The Postwar Rearmament of Japanese Maritime Forces 1945-71* (Praeger, 1973); Zbigniew Brzezinski, *The Fragile Blossom: Crisis and Change in Japan* (Harper & Row, 1972); John K. Emmerson & Leonard A. Humphreys, *Will Japan Rearm? A Study in Attitudes* (American Enterprise Institute & Hoover Institution, 1973); Herman Kahn, *The Emerging Japanese Superstate. Challenge and Response* (Prentice Hall, 1970), Kwan-Bong Kim, *The Korea-Japan Treaty Crisis and the Instability of the Korean Political System* (Praeger, 1971), Kiyoshi Kojima, *Japan and a Pacific Free Trade Area* (California, 1971), Lawrence Olsen, *Japan in Postwar Asia* (Praeger, 1971); Robert E. Osgood, George R. Packard III, and John H. Badgley, *Japan and the United States in Asia* (Washington Center of Foreign Policy Research of the Johns Hopkins University School of Advanced International Studies, 1968); and Robert Scalapino, *Asia and the Major Powers: Implications for the International Order* (AEI & Hoover, 1972).

For valuable criticism and editorial assistance, *arigatō* to Felicia Oldfather, Jon Livingston, John Dower, Steve Andors, Douglas Mendel, Alice Hsieh, and my wife Carol.

2. Interview with Mr. Herbert Levin, Deputy Director, Japan Affairs, Bureau of East Asian and Pacific Affairs, U.S. Department of State (November 28, 1973).

3. *Pacific Imperialism Notebook*, Special Japan Issue, 3:1 (December 1971 – January 1972), 18.

4. This issue is too complex for satisfactory analysis here. For readers who wish to probe further into it, I highly recommend *The War Economy of the United States: Readings in Military Industry and Economy*, ed. Seymour Melman (New York: St. Martin's Press, 1971), and *The Economic Impact of the Cold War. Sources and Readings*, ed. James L. Clayton (New York: Harcourt, Brace & World, 1970). Both are paperbacks.

5. The *Sanken*, as it is commonly abbreviated, has appeared as the dominant economic organization only since 1970. Chitoshi Yanaga's *Big Business in Japanese Politics* (Yale University Press, 1968) makes no mention of it. The only two discussions I have seen appear in the Special Japan Issue of the *Pacific Imperialism Notebook* cited above in Note 3; and in Kato Hidetoshi, "Sanken: A Power Above Government," *The Japan Interpreter*, 7:1 (Winter 1971), 36-42.

6. Other prominent Japanese writers on foreign affairs in the "new realist" school include Etō Shinkichi, Professor in the Faculty of Law at Tokyo University, and Wakaizumi Kei, Professor of International Relations at Kyoto Sangyō University.

7. Not all organized expressions of opposition to official policy originate on the political left. The *Seirankai* (Summer Storm Society), headed by Nakao Eiichi, is an influential group of conservative young LDP Diet members (mostly in their 40s) enjoying considerable support from their generation of the business community, which usually takes hawkish foreign policy positions to the right of the Tanaka cabinet. There are others as well.

8. Lester R. Brown, *World Without Borders* (New York: Vintage Book, 1973), 196.

9. Ibid., 245-48.

10. Liberal emphasis given to economic constraints generally does not extend to analyzing Japanese policy as part of an "international system." Hypotheses suggesting how events in Europe or in the Middle East or at the SALT talks might affect options open to Japan are rare. Rather, international constraints are most commonly portrayed as related to Japan's peculiar capacities, special problems and bilateral relations (again note the emphasis evident in lines 4A, B and C of Chart 1).

11. Peter Dale Scott, *The War Conspiracy* (Indianapolis: Bobbs-Merrill, 1972). See Moss Roberts' review in this *Bulletin*, 5:1 (July 1973), 69-73.

12. John E. Mueller, *War, Presidents and Public Opinion* (New York. John Wiley & Sons, 1973), 119, 130 and *pari passu*

13. John W. Finney, "Military Budget Spurs Economy," *New York Times* (February 27, 1974). Finney writes that according to Secretary Schlesinger's testimony on February 26, "next year's military budget was increased at the last minute by more than $1 billion by the White House to help provide a stimulus to a lagging domestic economy." He continues, "the acknowledgment that the size of the record $85.8 billion defense budget for the coming fiscal year had been dictated, in part, by domestic fiscal considerations and not strictly by military requirements introduced a new factor into the annual Congressional debate over defense spending."

A statistic commonly used by economists to determine whether a budget is expansive, neutral or depressing is the "full employment surplus or deficit." This means, for example, that the $295 billions of "budget receipts" projected for fiscal 1975 would expand to $311 billions were we to have "full" (96 percent) employment. Likewise, the projected $304.4 billions of "budget outlays" would reduce to $303 billions in the so-called "full employment budget." Thus the "full employment surplus" for fiscal 1975 would be 311 minus 303, or $8 billions. For the last three years this full employment surplus has been increasing.

Fiscal 1973 (actual)	- $2 billions (deficit)
Fiscal 1974 (estimated)	+ $4 billions (surplus)
Fiscal 1975 (estimated)	+ $8 billions (surplus)

Therefore, even with the high current allotments for defense, the Administration is able to credibly argue that its total budget is counter-inflationary. See President Nixon's "Budget Message," *New York Times* (February 5, 1974), p. 23.

In reality, though, the issue of the contribution of the defense budget to growth must be divided into two separate issues. First, the absolute weight of the defense sector probably is not replaceable with allotments to other sectors; that is, we could not cut the $85 billions figure to $40 billions and still achieve the same rate of growth. However, secondly, incremental "stimuli" of $1 to 2 billions easily could be spent on other federal programs with equivalent expansive effect.

The Chinese Peasantry and Imperialism: A Critique of Chalmers Johnson's Peasant Nationalism and Communist Power

by Elinor Lerner

Preface

Chalmers Johnson's book *Peasant Nationalism and Communist Power* is important for several reasons. It is a complete reinterpretation of events of the Chinese revolution, presenting the revolution as merely another form of nationalist development of the modern nation-state. And it has had an enormous impact in the academic community. Most of the major works on this period of Chinese history are to some extent answers to Johnson. And its effect has extended beyond the field of Chinese studies. Almost all other studies of peasant mass movements and contemporary social change at least acknowledge Johnson's thesis and make some attempt to deal with it.

I hope to show in this paper that the time is long overdue for freeing both studies of the Chinese revolution and other peasant mass movements from the spectre of Johnson. I intend to show that, on scholarly grounds, the book does not deserve the respect which it has received. But more important, my main point is that the discussion of Johnson's thesis should not center around the factual correctness of the book. Rather, the point of criticism should be the theoretical framework of the book. Even more insidious from the standpoint of intellectual activity than the endless debate on the importance of nationalism in peasant movements and the conservative reinterpretation of revolutionary movements, is the implicit theoretical framework and view of social change which one adopts in dealing with Johnson. What I hope to show is that debates which take place within the context of elite theory cannot possibly refute Johnson and that to understand the real flaws in the book one has to examine the relevance of elite theory for studying social change and mass revolutionary movements.

Like others who discuss Johnson's book I find myself using his categories and speaking in his terms. Thus I find myself talking about "peasants supporting the Communists," "Communist-controlled areas," and "Communists taking power." Use of this type of description implies acceptance of certain conceptual categories and social theories. For example, it implies that there exists a definite split between the leadership (elite) and the base (mass) in a social movement and that this split is in some basic sense unbridgeable. There is also ambiguity in my use of the word "communist." Sometimes it refers to the whole movement, sometimes just to party members. This ambiguity is due in part to a shifting between Johnson's framework and the tentative beginnings of one of my own.

The page numbers in parenthesis refer to *Peasant Nationalism and Communist Power* unless otherwise noted, or when the reference is clear from the text.

CONTENT OF THE BOOK, or IS JOHNSON RIGHT?

The Basic Argument

In *Peasant Nationalism and Communist Power* Chalmers Johnson seeks to explain why the peasants supported the Chinese Communist party (CCP) from 1937 to 1945. According to Johnson, the CCP became an enormously popular mass movement not because of its social programs but rather because of its anti-Japanese activities. Johnson states that the CCP led a war-energized, radical nationalist movement, with socialist ideology merely an adjunct to

Chinese nationalism (ix). The book is an attempt to show that Chinese communism was (and still is) a nationalist, not a socialist, movement (ix).

In this Johnson has completely reinterpreted events in China. If he is correct, then what occurred was not a mass-based communist revolution. It was, instead, an elite-led, nationalist movement to establish a modern nation-state. The different implications of these two analyses are clear. Thus the importance for assessing the correctness of Johnson's study.

On the surface Johnson's argument is quite simple. He argues that before the Japanese invasion the major CCP appeals were economic and social (e.g. land redistribution, justice for the poor), and that the CCP failed to gain mass support for these policies. He concludes that the Chinese peasants were not attracted to or motivated to action by social and economic issues (2)

The Japanese invasion of China created two related conditions which finally enabled the Communists to gain power through peasant mass mobilization. First, because of the invasion the situation in rural areas degenerated. The gentry fled, leaving villages with no leadership. Economic conditions were unstable, with the peasants experiencing even worse poverty and uncertainty than usual. Second, the particularly harsh practices of the Japanese against the peasants encouraged the formation of spontaneous local self-defense units (3-5)

According to Johnson, the Communists took advantage of this situation, abandoned their social and economic programs, and adopted a nationalist, anti-Japanese one instead. Under the banner of nationalism the CCP organized, coordinated and led the popular resistance movement against the invading Japanese. Thus Johnson explains the mass support for the CCP as being due to nationalist rather than revolutionary sentiment (7)

This argument hinges on several crucial assertions One is that the CCP had little success organizing around social and economic issues before the Japanese invasion Another is that during the anti-Japanese war CCP-controlled areas and popular support greatly increased. Johnson's argument depends heavily on demonstrating that during the anti-Japanese war the CCP gave primary stress to nationalist appeals as opposed to social and economic ones. Related to this is Johnson's assertion that CCP wartime social and economic policies were moderate in nature, stressing United Front politics rather than socialist revolution. Lastly, implicit in Johnson's analysis is the assumption that CCP nationalist appeals were directed toward gaining peasant support

The Perspective: Elite Theory

While Johnson's argument as presented above will be criticized on factual grounds, there is another more subtle analysis in the book which will be criticized on a theoretical level. Johnson states his aim to be the exploration of the nature of mass mobilization and peasant nationalism The subject of the book is the "resistance movement of the Chinese peasant against the Japanese" (vii). However, Johnson is actually much more concerned with the basis of CCP authority, power and legitimacy of ruling elites than with an analysis of mass social movements.

Johnson is open about his view of the Communists as elites. In the preface he states that he is studying the "rise to power of an elite organization" (viii). What is less apparent is that the fundamental theoretical framework of the study is standard elite theory. Johnson's book is not an analysis of a mass social movement but a description of a change in elites. The study is actually oriented around the standard questions of elite theory: how does an elite gain power and how does it legitimate its rule, quite different considerations from those involved in studying popular resistance movements!

Now let me restate the heart of Johnson's argument from the perspective of elite theory. Johnson claims that the rural gentry was driven from the countryside by the Japanese, leaving the rural areas without effective leadership and in a state of chaos. Johnson asserts that the CCP gained power by stepping into the vacuum caused by the disappearance of the rural elites. They did this by providing leadership and coordination for the indigenous anti-Japanese struggle. The CCP "offered to meet the needs of the people for leadership in organizing resistance to the invader and in alleviating war-induced anarchy in the rural areas."(7)

Thus Johnson gives the following answers to his main questions. The CCP gained power by military defeat of the Japanese (and later the KMT). It gained legitimacy through nationalism. Indeed, the centrality in elite theory of determining the legitimacy of new elites is stated by Johnson: "[one has] to give primary consideration to the circumstances under which it [an elite organization] achieves a mass following."(viii)

Outline of the Book

The book begins with a preface in which Johnson states his aims and major results. He seeks to explore (1) the political and military characteristics of the resistance, (2) the influence of the resistance upon the course of Chinese politics, and (3) the circumstances in which the CCP and the peasantry collaborated and the nature of rear-area governments.(viii) He states that the book is to be taken as a case study of a modern nationalist movement in communist disguise.(ix) In other words, Johnson claims to be studying peasant nationalism, not specifically the Chinese Communist rise to power. However, we shall see how odd all of this is in light of the fact that Johnson is really studying the *use* of peasant nationalism as a legitimating device for elites.

In the first chapter, "Peasant Nationalism in China," Johnson devotes only one page to expound on his major assumption of the lack of peasant support for the CCP before 1937 He does not prove it; he merely asserts it. "From 1921 to 1937 Communism failed in China because the Chinese people, in general, were indifferent to what the Communist Party had to offer."(7) This statement, and similar ones, are not documented. Then Johnson describes briefly his theory of the relationship between the Japanese invasion, CCP activity and nationalism. He does not prove his thesis here but states that "in succeeding chapters I shall detail the actual experiences that support such a theory."(6)

It is in this first chapter that Johnson rules out all other explanations of Communist success. The CCP and the mass movement were not *really* communist because one cannot prove that Mao took orders directly from the USSR; therefore the victory was not due to communist ideology. The movement was not economically motivated because peasants

have always been poor, so why should they care about it more now than before. And besides, "the Chinese Communist party did not offer to advance the economic interest of the peasants during the war."(18)

Johnson gives a brief (and, as we shall see inaccurate) description of CCP wartime social programs in which he characterizes them as conservative and then dismisses them as unimportant.

He ends the chapter with a discussion of nationalism and outlines the history of nationalism in China in which he refers primarily to the growth of nationalism in the elites – not in the peasantry.

What is particularly interesting about this chapter is the perfunctory manner in which other explanations are dismissed and the confusing manner in which the main thesis is stated. One gets the impression that the centrality of nationalism either has already been proved or soon will be In this chapter it is accepted as fact. Let us see if it is indeed proved elsewhere.

Chapter Two, "The Japanese Role in Peasant Mobilization," is concerned mainly with Japanese military matters and the impact of the Japanese invasion on China. Johnson describes, in military terms, the conquest, the establishment of a puppet government, the harsh policy of the Japanese toward the peasants and the Japanese military campaigns against Communist-held areas

The point of all this military information is given by Johnson at the end of the chapter

> *Japan's invasion and occupation of China decisively altered the political interests of the peasantry Prior to 1937, the peasants were a passive element in politics, even the earlier Communist bid for power, based on an appeal to peasant economic interests, was a conspicuous failure The prewar peasant was absorbed in local matters and had only the dimmest sense of 'China ' Japan's invasion changed this condition by heightening the peasant's interest in such concepts as national defense, citizenship, treason, legitimacy of government, and the long-range betterment of the Chinese state. This came about as a result of certain specific new pressures on Chinese rural society that were contributed by the Japanese Army (69)*

What were these pressures? "The hostile activity of easily identifiable foreign soldiers."(69) Hardly a unique occurrence in Chinese history!

This selection from the book is interesting because of the many distortions it contains Considering the many peasant rebellions, especially the Taiping, one can hardly call Chinese peasants passive politically before 1937 And the logic of the last sentences is confusing. If the peasants, prior to the Japanese invasion, were absorbed in local matters, where did the concepts such as citizenship and treason come from? How could they be "heightened" if they were not there already?

Even ignoring, for the moment, Johnson's historical distortions, does he show in this chapter how the Japanese invasion gave rise to this new awareness on the part of the peasantry? Unfortunately Johnson devotes only two or three pages total to a discussion of peasant reaction to the Japanese. He states that the peasants were really mobilized by the Japanese because of their brutality and not by the Communists, who merely profited from the spontaneous uprising.(49) However, Johnson does not prove or document this.

The third chapter, "The Communist Army," describes the development, organization and politics of the Communist Eighth Route Army While the heart of the chapter is concerned with the increasing size and the political nature of the army (particularly the internal political structure), this chapter, at least, does contain several pages devoted to the army's political work with the peasants. However, it is approached from the perspective of army organization and logistics, not peasant motivation. Thus we are now halfway through the book and there is still little evidence for the major claim.

The next chapter, "The Growth of Communist Controlled Territory in North China," details the founding and expansion of several Communist-controlled areas. The description is largely in terms of military conquests and not in terms of social, economic or political policy, much less in terms of peasant motivation. The following chapter, "The New Fourth Army and the North Kiangsu Base," is a similar description of Communist military expansion in South China.

The reader is now avidly awaiting the *dénouement* where all of this is brought together and the thesis of nationalism as the major appeal for the peasants is finally tackled. Alas, the next chapter is on "Peasant Mobilization in Yugoslavia." The last chapter, "Communism and the Nation-State," is devoted to a discussion of the as-yet-unproved result and its implications for later government policy. Usually it suffices, in criticizing a book, to show that the major assertions have not been substantiated. However, with Johnson the task has just begun.

FACTS?

Although Johnson's book is now a classic in Chinese studies it has often been criticized for its narrow approach and factual inaccuracies. Donald Gillin, in his review of the book in the *Journal of Asian Studies*,[1] chides Johnson for his "absence of adequate historical research"[2] and suggests that in the absence of such, "social scientists must beware of making generalizations concerning events in China during the 1930s and 1940s." But Johnson's study is not only historically inaccurate, it is also profoundly a-historical.

Moreover, it is also a-social in the sense that it contains no accurate social or economic analysis For a work dealing with a mass social movement and the motivation of its members, it is noticeably lacking in information about those members and the social, economic, and political conditions of their lives. The Chinese peasants are defined and discussed primarily by their relationship to the Japanese That is, there is nothing in Johnson's presentation which points out the particularity of the Chinese peasants. In the discussion on elite theory it will become clear that this is not an oversight on Johnson's part, but rather stems directly from the theory. what matters are the external forces (the Japanese) and the elites (the CCP); the base is largely irrelevant except for its existence.

Peasant Support: South China

Johnson's argument depends heavily on the CCP having little popular support before the Japanese invasion. Since, before 1937, Communist appeals were obviously based on

social and economic issues, with programs centered around demands such as land redistribution and equality for women, this helps to establish his claim that peasants were not then (and therefore would never be?) attracted by socialism.

But this is simply not true. During the Kiangsi Soviet, 1931-1934, the CCP controlled a definite geographical area and population of about three hundred counties (concentrated in three provinces) and thirty million people.[3] Only after a massive military effort was the KMT able to drive the CCP and many of its supporters out of Kiangsi.

Johnson devotes only a few scattered paragraphs to this period and states that "the peasantry collaborated with the CCP half-heartedly," and that the "relationship between the two [peasant and party] was contingent on military success."(2) This is not true. The peasants fought willingly in a volunteer army of 150,000.[4] Peasants joined the party in large numbers – by 1933 there were 97,451 members in Kiangsi province. According to Hofheinz, this number, 4 percent of the population of the province, cannot be ignored as an indicator of peasant mobilizaiton.[5]

Then belying his own claim that the peasants were not attracted by social or economic programs, Johnson states that the CCP "purchased peasant support . . . with their anti-landlord policy."(2) By this statement Johnson is negating his assertion that peasants were never attracted by economic concerns. However, it is not really a contradiction to him since his use of the word "purchased" seems to imply that the peasants were not really acting for economic reasons because the *motivation of the CCP leaders* was impure!

According to Ilpyong Kim in his article on mass mobilization during the Kiangsi period,

> *One of the most important organizational goals of the Chinese Communist movement during the Kiangsi Soviet period (1931-1934) was mobilization of the broadest possible mass participation in the revolutionary processess of the Chinese Soviet Republic. . . . By emphasizing the central concept of "mass participation" the soviet government achieved considerable success in mobilizing the population in the process of implementing its newly formulated social and economic policies in the soviet areas.*[6]

The article goes on to describe in detail the extent, techniques and relative success of mass mobilization around social and economic issues. Kim is especially concerned with the "land campaigns, and the establishment of the poor peasant crops and the farm labor union."[7]

While it is definitely correct that the CCP did not have the same type of relationship with the peasants during the Kiangsi Soviet that they later had in North China, this was not due so much to lack of appropriate appeals and ideology as to the relative youth, inexperience and early development of the CCP organization and understanding of mobilization techniques. Johnson claims that the Communists used the same organizational techniques before and after the Japanese invasion, the relative success and failure being due to the appeal which they were servicing.(1) This is not correct. While the general attitude toward mobilization and peasant participation was the same (as the Kim article shows), Schurmann and others have shown that the greater success in North China was due in large part to the conscious effort by CCP leaders to correct and improve on methods used in Kiangsi.[8] And along with the mistakes in organizational technique, Selden gives other reasons for the military defeat of the CCP by the KMT:

> *The failure of the revolutionary movement to sweep to national victory during the Kiangsi period must be ascribed less to the inherent lack of appeal of agrarian revolution than to the strength of the forces of counterrevolution, the landlords and warlords, and to internal conflicts within the movement.*[9]

Peasant Support: North China

Just as Johnson is incorrect about lack of peasant support for CCP economic and social programs in South China prior to the Japanese invasion, he is also incorrect about the situation in North China. Mark Selden, in his doctoral dissertation, has shown that there was considerable Communist activity in North China beginning in the late twenties. Several Communist-led peasant uprisings occurred before 1934. On June 6, 1932, a Soviet was created in three districts in the southern part of Shensi province, and land redistribution was begun.[10] Selden notes that the success of these early efforts was hindered by too moderate a form of land redistribution (rich peasants were heavily favored) and by military realities, the area being "peculiarly vulnerable to attack by government forces."[11] These two factors, rather than lack of interest in CCP programs, kept popular support in check. Nevertheless, by 1933 party membership in Shensi province was 3,000.[12] In the summer of 1933 this Communist and peasant movement was crushed by massive military attacks from the KMT.

In late 1933 the movement reappeared in northern Shensi. By February 1934 northern Shensi had a provisional Soviet government and Communist guerrilla forces were again scoring small victories against the warlords and the KMT.[13] Soon the Communists had over 3,000 guerrilla fighters and much peasant support.[14] The KMT launched military campaigns against the Communists but due to their good location the Red Army was able to withstand the attacks. Selden attributes the greater success of the Communists in northern Shensi to the logistically better circumstances for guerrilla warfare and to the more oppressed condition of the peasantry.

In January 1935 the First North Shensi Soviet Congress was held.[15] During 1934 and 1935 a land redistribution program was widely implemented for the first time in North China. With the first arrivals from the Long March, the total Red Army strength in North China was fourteen to fifteen thousand.[16] The above account is hardly that of a situation in which the Communists had no peasant base.

Selden shows that by the time of the Japanese invasion the Communists had large areas, were organizing them with government structures, had instituted land redistribution, controlled large volunteer armies, and were defeating both the warlords and the KMT in battle.

Johnson did not have to wait until Selden's research appeared to discover that his crucial assertions were incorrect. In his review of *Peasant Nationalism and Communist Power* Donald Gillin also documents CCP activity and peasant support for CCP policies before the Japanese invasion. Certainly the information and sources available to Gillin were also available to Johnson.

Gillin presents in great detail and with much

documentation CCP activitiy in Shensi between 1935 and 1939. He begins by noting:

> *Since 1927 there had been many communist sympathizers among the students attending middle schools and colleges in Shansi, but by 1935 Yen Hsi-shan [the warlord who ruled Shansi] was considerably more alarmed about the increasingly radical outlook of the peasants who made up the vast majority of Shansi's thirteen million people. Because of adverse conditions created by the world depression, as well as the heavy taxes which Yen imposed in order to sustain his ten-year program of economic development, the peasants of Shansi were desperately poor and therefore ready to welcome the Communists in the hope that any change would be for the better. This explains why small bands of Communist infiltrators succeeded in virtually taking over several districts lying along the southwestern border opposite Shensi. In the fall of 1935, Yen Hsi-shan admitted that the Communists enjoyed the sympathy of 70 percent of his subjects and warned that if the Red Army entered Shansi, it could recruit well over one million men from among the poor peasants and the unemployed. . . . He was less frightened by the military strength of the Communists than by the probability that the radical social and economic reforms which the Communist Party advocated would cause his own people to join forces with the Red Army against him.*[17]

This hardly indicates that the CCP lacked support in North China before the Japanese invasion. Furthermore, Gillin indicates in the above passage that peasants were attracted to the Communists not from nationalism, but because of deteriorating social and economic conditions.

Gillin goes on to describe how easily the CCP gained control of large parts of Shansi province.

> *After invading Shansi in February 1936, the Red Army enjoyed what must have been massive popular support, because within less than a month the outnumbered and ill-armed Communists succeeded in occupying nearly a third of Yen Hsi-shan's domain. Only two-thirds of the 34,000 Communist soldiers who invaded Shansi had rifles, whereas Yen's 70,000-man army even commanded the service of a fleet of warplanes.*[18]

Gillin notes that the Communist troops were using guerrilla tactics which also implies much support from the population.

CCP Policies During the Anti-Japanese War

Not only does Johnson (erroneously) assert that the peasants were not and could not be mobilized by radical social and economic programs before the Japanese invasion, he also claims that after 1936 the CCP did not even attempt to do so. According to Johnson, during the anti-Japanese war the Communists' social and economic programs were conservative to moderate and little attempt was made to organize the peasants around these issues.

> *The Communists themselves took no chances on repeating their failure to unite the masses in the Kiangsi Soviet; they eschewed their old slogans of class warfare and violent redistribution of property in their post-1937 propaganda and concentrated* solely *on national salvation* [4, emphasis added]

Johnson's Treatment of CCP Policies During the Anti-Japanese War—A Digression

Johnson represents CCP economic and social programs as being conservative and directed largely to the gentry. Johnson bases this interpretation on two misconceptions and one omission. The first of the three errors is the false impression that CCP programs remained constant throughout the anti-Japanese war and uniform throughout all CCP-controlled areas. The second misrepresentation is due to Johnson's a-historical approach. He ignores the evolution of CCP strategy, particularly with respect to land policy. Thus he fails to understand the meaning of "moderate" United Front policies. He misrepresents programs both in relation to past policies and in relation to their present social contexts. Lastly, while he mentions those economic programs such as rent reduction, which could most easily be interpreted as moderate and directed at gaining gentry support because they guaranteed gentry interests, he barely mentions other programs, such as the cooperative effort, the drive for mutual-aid teams, which were clearly directed toward improving the economic condition of the peasantry and which were quite radical in nature.

Johnson arbitrarily picks one program which officially remained constant throughout the period and then treats all programs as if they did the same. He also assumes that programs were applied uniformly in all areas. A minimal knowledge of the period shows that this is false. In February 1937, in accord with the United Front policy, a new land policy was adopted. It lasted, officially, until 1946. Under this law, land confiscation was suspended and a wartime program of rent reduction and reduction of interest rates took its place. However, the application of this policy varied greatly from area to area and from time to time. In fact, it was rarely put into practice until 1942, when the Politburo issued a new pronouncement on land policy. This new policy called for rent reduction coupled with a new political economy involving cooperatives, mutual-aid teams and rural industry.[19]

While land policy officially stayed constant from 1937 to 1946, in effect it did not. There were changes in application, introduction of other policies relating to the policy of rent reduction, and all of these policies had different effects depending on the past history of the area of application.

In fact, one can show this by citing references from Johnson's own bibliography. On a theoretical level the *Selected Works of Mao Tse-tung* contains selections presenting the changes in policy and explaining the politics behind these changes.[20] Chao's book, *Agrarian Policy of the Chinese Communist Party, 1921-1959,* a standard reference in the field, details changes in CCP land policy and practice and relates these changes to changes in the political, economic and military situation.[21]

As to the uniformity by area, it is astounding that Johnson does not deal with the different practices in different types of areas. Mao often distinguished at least three different situations requiring different tactics. In the article "Problems of Strategy in the Guerrilla War"[22] he divides areas into secure base areas, areas which pass in and out of Japanese hands and those firmly behind Japanese lines. Obviously the Communists could institute more radical policies and implement more structural changes in secure base areas than in those which

might pass back into Japanese control and hence expose the peasants to retaliation. For example, many landlords collaborated with the Japanese. Thus it was harder to get peasants to attack landlords if the peasants felt it was likely the Japanese would recapture their village and reestablish the landlords' power.

Not only does Johnson arbitrarily pick one set of policies, rent and interest reduction, claiming that these (and therefore all others) remained constant from 1937 to 1945, he also misrepresents their significance by not placing them in a social or historical context. However, for Johnson this is not a problem since he treats programs and policies as if they have certain *innate* politics.

Ignoring contrary information in Chao's book, Johnson represents CCP land policy before 1937 as being one of continual land redistribution and hence radical. After 1937 there was a policy of rent reduction rather than land distribution. Johhson picks this up as the main economic policy of the period and declares that this and therefore all economic policies were conservative. Johnson makes the common error of assuming that land redistribution is necessarily revolutionary. But this is not so; land redistribution can also lessen the revolutionary potential of the peasantry and can be used to gain support for conservative governments.

The U.S. occupation forces in Korea enacted a mild (and ultimately ineffective) land reform program in order to win support for themselves and the U.S.-backed government. The KMT in Taiwan instituted land reform to eliminate some grounds for the Taiwanese organizing against the KMT occupation. The effect of both these land redistributions was to increase the stability of reactionary governments. There is nothing intrinsic in a land program that predetermines its politics. Rather, the politics are determined by the social and historical context. One needs to know who is carrying out the program and how it is related to other social and economic policies.

In this light, it is interesting to note that it is also true that policies which begin as radical often end up as potentially conservative. For example, land redistribution sometimes contributed to the conservatism of the beneficiaries by making them less likely to favor further changes in land policy such as collectivization.[23]

If one views the 1937 rent and reduction campaigns in the context of past land policies, one can see that it was not necessarily an unexpected change, nor did it indicate a shift to conservative politics. A careful study of CCP land policy shows that the Chinese Communists have alternated between periods of redistribution which usually caused production and social disruption, and periods of consolidation and stabilization. Before the United Front the Communists had conducted a massive land redistribution program. So, if there was to be a change at all, a return to a milder policy was to be expected.

In areas where partial land redistribution had taken place there was some resistance from peasants and lower-level cadres to what they saw as a policy of class conciliation. But in areas which had been newly taken from the Japanese, rent and interest reduction was a genuine help to the peasants. Gillin analyzes the effect and meaning of the interest reduction program. He notes that in some areas in North China tenancy was not very widespread and that usury was actually more of a problem.

> *Because of the severe depression which afflicted agriculture in Shansi throughout the 1930's, all but the richest peasants were compelled to borrow money . . . A native of Shansi recalled that by 1937 96 percent of the families living in his village were in debt. Since this clamor for loans coincided with a growing shortage of specie, interest rates rose rapidly, until by 1936 they averaged 50 to 60 percent. . . . Thus, there seems to have existed in Shansi a great deal of 'disguised' tenantry, in the form of millions of nominally independent yet in reality permanently indebted peasants whose suffering at the hands of money lenders was comparable to the exploitation endured by most tenant farmers. This is why the peasants remained unenthusiastic about Yen Hsi-shan's efforts to initiate land reform but often rioted when banks and money lenders tried to collect the sums owed them. Consequently, by practically banning usury in Shansi the Eighth Route Army carried out a social and economic revolution in the utmost magnitude. . . . This policy had the effect of wiping out the chronic indebtedness of the vast majority of the population.*[24]

But Johnson gives no hint of this. He could not, since he gives no description of the actual conditions of the peasantry, such as the relation between tenancy and indebtedness. But the reason that he does not feel it necessary to go into these matters is precisely because they are irrelevant for him. If policies innately have their own politics irrespective of the social context, then why bother analyzing the historical and social situation in which these policies occur?

Economic and Social Policies During the Anti-Japanese War

While asserting that he is studying Communist strategy for organizing peasants, Johnson ignores many facets of Communist activity, primarily those directed toward the peasants. Communist programs during 1937-1945 showed a complexity not even hinted at by Johnson. The rent reduction program, for example, did not exist in isolation. Rather, it was intertwined with production and political campaigns aimed in part at beginning to alter the basic structure of village organization and the political and economic life of the peasantry.

Indeed, Johnson's work is often criticized on these grounds. Gillin states that:

> *Although the Communists refrained from openly redistributing the land, they introduced a host of other reforms which revolutionized the existing social structure in Shansi by undermining profoundly the power of the rich.*[25]

He then goes on to describe many of these policies and the revolutionary effects that they had on peasant life. He presents Communist organizing tactics around the war effort as a means to undermine the power of the rural gentry. For example, the Communists went directly to the poor peasants in setting up leagues to support the war effort and in this way created structures which gave the peasants power and which took away from the power and authority of the rural gentry. Under the slogan "Those having wealth must contribute money; those with muscle, their strength," many campaigns were conducted which compelled the gentry to donate large sums to the war effort. If they resisted they were denounced publicly and their wealth was confiscated. Much of this resembled peasant participation in the earlier land reform campaigns.

This whole process had profoundly revolutionary overtones. Under the guise of financing the war effort, the Communists probably confiscated much of the wealth possessed by the gentry. Apparently the gentry often lost everything. . . . The Communists justified their treatment of the gentry by accusing them of having obtained their wealth through exploitation. Several observers have described the intense enthusiasm which this policy of taxing the rich instead of the poor aroused among the peasants. One infers that it was the chief reason for the enormous popularity of the Eighth Route Army in Shansi.[26]

Gillin also notes that many important social reforms were carried out "in the guise of helping the relatives of peasants serving in the armed forces."[27] Massive education and medical care programs were introduced. The social position and power of women, particularly those whose husbands and sons were in the army and who were thus the heads of households, were radically altered.

Likewise, Mark Selden in his doctoral dissertation, by concentrating on Communist activity in areas outside of Japanese attack, considers economic and social policies hardly touched on by Johnson. The major portion of Selden's work details the various campaigns which began to revolutionize the social life in the countryside. In particular, his last chapter, "Yenan Communism," presents in detail the social and economic programs of the CCP from 1937 to 1945.[28] For example, in this chapter Selden describes the cooperative movement which was designed to set up rural agricultural and industrial cooperatives. Aside from the concrete benefits in terms of additional productivity, these cooperatives were intended to initiate changes in rural production from individual, private-ownership methods toward collective, communistic methods.

He describes the "To the Village" movement in which all party, army and government officials were to actively participate in rural agricultural work. While this also was designed partly in order to increase productivity, it also had other, more far-reaching consequences. It set a precedent for the many other such campaigns (including the recent cadre schools set up during the Cultural Revolution) and it helped to break down the traditional distinctions between officials and peasants. It also was designed to destroy the traditional attitude toward manual labor as demeaning. Similarly, the Labor Heroes campaign was an attempt to alter attitudes toward manual labor and to introduce a new, social meaning for labor. It also had the effect of creating a new peasant leadership from the class of poor peasants.

The Election Campaigns

The election campaigns of 1937 and 1941 were relatively simple in intent: to set up popularly elected governments from the village to the Border Region level.[29] Originally planned during the few short years of the United Front, these governments were to contain representation from the broadest possible range of anti-Japanese groups. A policy of Three-Thirds was devised to insure equal representation of the three major political forces: elected officials would be approximately a third each from the party, the peasantry and the gentry. Implementation consisted of cadre teams visiting villages, contacting local leaders, working through and creating new mass associations in order to inform peasants about the elections. Any legal anti-Japanese group was allowed to nominate candidates and any adult who was not overtly pro-Japanese was allowed to vote.

In 1941, after the second Border Region election, 1,549 townships had elected about 30,000 local council members. The election campaign was usually completed only in those areas secure from Japanese attack. In less secure areas fewer elections were held or governments were appointed temporarily by local CCP or mass association members.

The election campaign had at least three major purposes: to create a functioning government, to unite the broadest possible segment of the population into an anti-Japanese coalition, and to create new forms of rural power and to redistribute old forms of power, that is, to alter the power distribution among the various classes by the institution of new political forms. Johnson ignores the first, discusses the second, and completely misunderstands the third.

The first purpose is often ignored by academics. Clearly the CCP needed a government structure to coordinate and solidify administration in the Border Region. A relatively large bureaucracy had developed and some form of central administration was necessary.

Johnson places most emphasis on the second purpose, the creation of a broadly-based united front, although his treatment of this is sketchy. For example, when discussing the government in Wut'ai he merely spends a page outlining which personnages held which positions and what official services the government performed. His analysis is not in terms of which groups held which positions, how the governmental power was balanced between the local classes or what changes in the rural structure this coalition introduced. Thus Johnson cannot possibly understand the importance of the elections. (101)

Originally devised under the United Front, the Three-Thirds system was partly a means of incorporating elites into a government along with peasants and the party. The ideological appeals to the rural elites were in terms of nationalism – all were to unite to defeat the Japanese. This strategy had many functions. It was a concrete manifestation of the United Front and as such represented good faith to the KMT and the national bourgeoisie. It created sympathy for the communists among the national elites and foreign "friends of China." It brought people with expertise and training into the government. And, lastly, by uniting a wide spectrum of classes and groups it helped to eliminate (temporarily and at one specific level) sources of opposition to the Communists. Thus it provided a stronger offense and defense against the Japanese and aided in creating a stable situation in which the Communists could pursue other social and economic policies.

By ignoring the first and third purposes, Johnson is able to concentrate on the coalition, United Front aspects, as opposed to dealing with the conflict politics of the election campaign. So Johnson can represent the campaign and the Communists' part in it as moderate to conservative. He can also stress the appeals to nationalism, which in this case were clearly directed as much, or more, to the rural elites as to the peasants.

Johnson makes the following comment on the Three-Thirds system.

Unity between the peasants and the Party was not based on the three-thirds system, because the peasants actually supported the Communists through the mass organizations

and the army. The three-thirds system was a device for incorporating local non-communist leaders, landlords, rich peasants, and other well-known people into the regional governments (14).

This is correct as far as it goes. Unfortunately it is the only reference which Johnson makes to the political, power significance of the elections. What he has done is to equate the elections with the three-thirds policy. Since this *policy* was not aimed at recruiting peasant support, he is then free to eliminate the *elections* as a possible source of peasant support for the Communists. What this means is that he confuses a specific policy with the elections and then dismisses both. Johnson's reference for the first statement in the above quote, which the reader probably hopes will justify and explain the support, is unfortunately to a U.S. Government War Department document and the footnote contains a long quote as to how the CCP kept tight control over the elections and how undemocratic the elections were. Therefore the reader does not get any further insight into the basis of the peasant-Communist alliance.

Johnson does state that the two functions of the elections, the creation of a stable government and the creation of a United Front, were not the most important for the peasantry. But he neglects to mention that for them the significance of the elections lay in the creation of new forms of local political power and the redistribution among the rural classes of the old forms of power.

Johnson does not overlook this completely but comments only on the psychological effect. (Thereby leaving the impression that the peasants only *felt* more involved, but actually were not. That is, the peasants were duped.) Johnson states that "the *feeling* of belonging and having a stake in government that grew up in this period was entirely novel to the Chinese masses; and it brought with it an exhilirating *sense* of self-determination." (p. 3, emphasis added) This quote is preceded by the claim that the elections were not democratic (not documented by Johnson) and hence the implication that the peasants did not gain any real power from the elections, and therefore that the elections were not significant in gaining peasant support. Johnson also leaves one with the impression that the elections were mere window dressing for a basically conservative United Front policy.

It is expected that Johnson would concentrate on the psychological effects of the election campaigns rather than the redistribution of power. This stems directly from his belief that social change really takes place in the form of changing elites. For Johnson nothing really changes in terms of the power of the ruled. The only significant question is how the new elites convince the ruled to grant them legitimacy. Hence the stress on how the peasants felt about the elections.

But Johnson is selling short the election campaign. The election campaign helped to create a new form and a new basis for political power in the countryside. Before the elections there was, in effect, little formal governmental structure in the villages. The national government stopped at the county level and the peasant rarely had contact with it. Most internal village affairs were handled by local institutions and organizations, such as the clans, or settled individually by landlords and gentry whose power was ultimately based in the ability to impose economic and physical sanctions.

Thus replacing the old political system was not a question of destroying a formal political system and instituting a new one. Rather, the task was to destroy a whole social system and a complicated relationship between classes in the village. This type of political system could not be tackled above the village level. In each village the Communists had to destroy the power of the local leaders and had to keep them from regaining power in the new political forms. Traditional village political power was based on an interaction of landholding, wealth, education and connections (official or otherwise) with the national government bureaucracy.

After the elections formal political power came from memberships in groups based on economic criteria such as class and occupation and on political criteria such as anti-war work. Ideally one was elected to office on the basis of services to one's group and was responsible to the group. In this regard, it is significant that candidates had to be nominated by groups; unaffiliated individuals could not run for office.

Thus, although many of the traditional elite held positions in the new government, it is critical to note than formally the base of their power had changed. Their formal political position was no longer based on land owning or connections to a national elite (although informally these still existed). The form of political relationships had changed. Even where the traditional elite retained positions of power, these were positions in a new political structure. In the new government most political interaction was still on a face-to-face basis at the village level, but the criterion for decisions had changed. Thus, the political official was now a representative of an organized structure, not representing only personal interests. Whereas before there had been KMT or imperial officials in the districts, they rarely had any contact with the peasants. Now government officials were local villagers and were in daily contact with the peasants. Now peasants dealt with their officials in practically all aspects of their lives, which gave a political cast to their existence.

Consequently the elections did not simply replace one elite with another. Peasants were attracted by the elections not because of nationalist appeals but because for the first time the elections gave them formal, institutionalized political power.

Nationalism

But what of nationalism? Didn't the peasants support the Communists because of their nationalism, created spontaneously by the Japanese invasion? Even given the previous criticisms of Johnson, although he does not prove this, it still could be true.

The question of the place of peasant nationalism in the Chinese revolution is more problematic than Johnson presents it. As is usually the case with mono-causal explanations it has some validity. Many peasants were aroused and did join the Communists because of the Japanese invasion. And the party did make heavy use of nationalist propaganda during the war. But as a blanket explanation it won't do.

First of all, Johnson oversimplifies the question. Clearly nationalism and anti-Japanese feeling are not the same. Johnson overlooks any distinction between them, and in fact gives an operational definition of nationalism which makes them identical. (ix) Johnson does not take into account the varying degrees of nationalism in the different classes or in

different areas (e.g., in and out of Japanese control). And he does not deal satisfactorily with the relationship between the spontaneous anti-Japanese feeling in the peasantry and the nationalism encouraged and organized by the CCP. He says that CCP activities

promoted mobilization in the countryside and, at the same time, fed upon the spontaneous peasant unrest. Nationalistic propaganda from Communist sources fell on fertile ground, where it both furthered the mobilization of the masses and helped determine the form this mobilization took. (4)

While in many instances peasants did form local resistance leagues, this was by no means universal. In fact, in no account relating village activities can I recall a case of the majority of the villagers organizing and fighting the Japanese before the arrival of the Communists. For example, in *Fanshen* no overt widespread resistance took place. Rather, a few villagers joined the underground. Likewise in the Chinese novel *The Hurricane* resistance was initially limited to a very few.[30]

Nor in villages where resistance took place did it necessarily represent the sentiment of the majority of the villagers. In *Fanshen,* where only a few villagers took part in underground activities against the Japanese, it did not seem to win them village approval.[31] After arrival of the Communists and after those who had been involved in anti-Japanese activities had also been involved in land redistribution, it was not at all clear on what basis they gained their local power. When it came time to elect peasants to leadership positions they were selected by the villagers primarily on the basis of their efforts in land redistribution. While the Communists praised them as gallant fighters, the peasants remembered their newly acquired household goods and voted accordingly.

Peasant support for the anti-Japanese war varied by area. In areas outside Japanese threat or control, CCP strength was due largely to economic and social policies. In these areas the Communists had trouble arousing support for United Front nationalist politics. In areas attacked by the Japanese, appeals to nationalism were more readily heeded. Because Johnson works mainly from Japanese intelligence reports he concentrates on areas which were directly involved in fighting the Japanese. Anti-Japanese feeling was higher in the peasantry in these areas than in areas further removed from the actual fighting. Nevertheless, even in these areas resistance usually consisted of small undercover units. Villagewide resistance, such as systematic hiding of grain and whole villages fleeing to the mountains until the Japanese left, was usually organized by the CCP.

In many areas, especially those removed from immediate fighting, the CCP had great trouble arousing the enthusiasm of the peasants for the anti-Japanese war. The many programs and appeals to nationalism cited by Johnson as evidence of peasant support for nationalism can more accurately be viewed as necessary efforts to create a support which was originally lacking. For example, the "Love the Army—Serve the People" campaign was designed specifically to perk up waning army-peasant relations. If peasants were so deeply imbued with anti-Japanese feeling, what would have been the necessity for such a huge effort?

The Communists had to offer many incentives to get the peasants to support the war effort. After all, why should peasants on the edge of bare subsistence give time or food to support an army and a war? Since membership in the Red Army was voluntary and since the army never lacked for recruits, obviously there was support for the war. But much of this was due to extensive propaganda drives and incentives for the families of soldiers. Periodic drives were made to gain recruits from villages and local political power in mass associations occasionally rested on being able to produce such volunteers. Families of soldiers were given tax benefits, Mutual-aid teams worked for the families of soldiers. During the United Front any family could qualify as anti-Japanese if it had a son in the army. And many soldiers joined for reasons other than nationalism.

Evidence that the peasants did not support the war and the Communists solely from nationalism can be gotten from the peasants themselves. Johnson claims not to have any peasant accounts.

Like all illiterate populations in such circumstances, the Chinese masses themselves — the peasants — have left no such records of the transformation wrought in their lives and thoughts. . . . This study attempts to reveal the nature of this transformation as it is unwittingly disclosed in the archives of the Japanese government. (5-6)

But Johnson is wrong. Peasants left much evidence in the form of interviews, both with Chinese and Western observers. Many of these accounts are contained in books in Johnson's bibliography.

As Gillin points out, when Edgar Snow asked peasants why they supported the Communists they usually did not give anti-Japanese or nationalist reasons. Instead they talked of the good things that the party had done for the poor peasants or of the good conduct of the Red Army. In a chapter in *Red Star over China* Snow details a conversation with several peasants. Their reasons for supporting the Communists are the better conduct of the Red Army, the opportunity for education, and relief from the landlords.[32] Gillin has many examples from Chinese sources. He quotes a woman as saying " 'You are the good army that doesn't harm people or do evil things . . . You are the Red Army,' "[33]

Besides appreciating the discipline and thoughtfulness of Communist soldiers, the peasants also supported the Eighth Route Army in the hope that it would carry out social reforms . . . A reporter who accompanied the Eighth Route Army in Shansi says that it owed its instant popularity among the peasants to the reputation which the Communists had earned in Shansi the preceding year. As one villager put it, "We know you're the Red Army, the army of us poor people." Several eyewitness accounts leave the impression that most people were overjoyed by the arrival of the Eighth Route Army simply because they thought it would attack the rich.[34]

But even if Johnson did not want to rely on such interviews, there are other ways to deduce reasons for peasant support.

Selden, after a careful study of Communist documents describing party growth and programs in a town in North China, concludes:

On one point the report is unequivocal. As late as 1939 after three years in which united front was reiterated as a fundamental principle, only one person out of the total of 134 had joined the party primarily to resist Japan, and that

was in 1937, long after Communist control and land revolution had been consolidated.[35]

Most of the others joined because they had profited from the recently concluded land redistribution campaign.

But then, if the peasants were not the nationalists, who were? Where did all this patriotism come from and to whom were the Communists appealing with anti-Japanese appeals? The answer is the national bourgeoisie, the national elites and the rural gentry. As Gillin says, "the Communist Party seems to have assumed an anti-Japanese stance in order to impress the privileged classes, whose power and vested interests were threatened by Japan." (274) The appeals to nationalism got their first support in the countryside from the local elites and during the United Front the nationalism appeals were directed primarily at getting local gentry support for the CCP-KMT united effort. Selden states:

[The] United Front was crucial in winning the support, or at least the tolerance, of the urban bourgeoisie and substantial segments of the landlord-commercial elite in the countryside. But such appeals were effective in securing active peasant support only when linked to a program focused on rural problems.[36]

But Johnson would have a lot of trouble recognizing this because central to the thesis is his contention that the rural gentry fled when faced with the Japanese. So how could Communists appeal to people who were not there?

With the Japanese invasion many elites did indeed flee their villages. However many of them stayed. Of these, some openly and actively cooperated with the Japanese, some joined the resistance and some quietly tried to save their skins and their money no matter what. (Also some did all three.) Johnson's main trouble here is that he makes no distinctions between types of rural elites.

Considering that one of the main intellectual debates in the China field for many years has been over the composition of rural and national elites it is astounding that Johnson never confronts the issue, but instead merely talks about rural elites as if they were a unified, homogeneous group. But, considering that Johnson has given no social or historical analysis of rural structure or class composition, it is not surprising that he can not distinguish between elites.

The Communists as New Elites – A Critique

In a study dealing with why peasants joined a particular social movement it is scandalous to find absolutely no description of peasant life, the mode of agricultural production, rural economic and power relationships, village social and class structure. For example, Johnson's assertion that rent and interest reduction are non-radical is actually meaningless without any indication of what land tenure systems existed, what rents were, to whom and how rents were paid, what interests rates were and how many peasants were in debt. Without such a social context, programs such as rent reduction become mere abstractions in any analysis.[37]

Johnson's account is totally void of why peasants would support anyone, especially the Communists, except for the brutality of the Japanese and the social disruption caused by the war. But these are conditions which would *add* to the revolutionary potential of the peasantry, they could not *cause* it. And given this potential, Johnson's analysis gives no indication of why its expression took the form it did, except that the Communists were there and were competent organizers. And in fact Johnson does indicate that it was in some ultimate sense accidental that the new elites were the Communists.

In their efforts at defensive organization, the villagers welcomed whatever capable military and political leadership they could find – Communist, Nationalist, Sacrifice League (Shansi), secret society, KMT Army remnants, or purely local leaders. (3)

In the logic of this argument one would have to conclude that the Communists won because numerically they had more competent leaders than anyone else!

Thus Johnson's seemingly superficial explanations of why the peasants supported the Communists. Johnson states that the peasants were unified and politicized as a concomitant of the drastic restructuring of life that accompanied the Japanese conquest. (ix) Johnson gives no explanation or description of how this unification and politicization occurred. But he does indicate what he means by a drastic restructuring of village life. Here he does not refer to changes in structure, methods of production, or the social situation of the peasantry. Rather, he is talking about what happened to the rural elites.

Johnson asserts that the local KMT and village officials fled from the Japanese, leaving a power vacuum in the countryside, thus setting the stage for the appearance of new elites. Johnson argues that there was no longer a rural elite, hence no ruling class, hence social disruption and dislocation. Peasants were consequently feeling an acute lack of leadership. Into this situation the Communists appeared, offered to exercise leadership, and thereby gained legitimacy by coordinating the anti-Japanese struggle.

There are several distinctions that can be drawn from this argument. One is that any vaguely competent, slightly nationalistic group that was organized, hardworking and clever enough could have assumed control. This leaves one with the "accidental" theory of the rise of communism in China: the CCP just happened to be there and be insightful enough to see a good chance when it appeared. It follows that the socialistic aspect of the CCP was really irrelevant. From this one can only deduce that the Chinese peasants really have been duped into communism. In fact, Johnson states that the present legitimacy of the CCP is due to their anti-Japanese activities. (10) He states that the Chinese peasants were willing to go along with later socialist programs because they had faith in the Communists who had led them against the Japanese. In other words, the peasants only support the economic and social policies because of the authority of the Communists gained through the anti-Japanese struggle. This is a fancy way of saying that the Chinese peasants have been manipulated and coopted. Another corollary is that the Communists couldn't have won if not for the Japanese invasion since it was the war which caused the evacuation of the old elite. (The haphazard theory of history?)

This last corollary deserves elaboration. In Johnson's analysis the explanations for change come from *outside* the system. He cannot and does not explain events by looking at the internal dynamics. There is no way, within his framework, to understand events in China by considering contradictions within the society itself. There is thus no way to understand

social change as a process, taking place over long periods of time and involving the active, conscious, deliberate rationality of people. Clearly, this implies that history is, to a large extent, beyond the control of people. So the social sciences are to some degree merely descriptive, and long-range political planning (as of revolutionary movements) merely illusory.

In back of Johnson's argument on changing elites is the assumption that the disappearance of the rural elite (which we have seen is somewhat overstated) created a *need* on the part of the peasantry for a new leadership group – that the peasants *required* a ruling elite. Yet since Johnson stresses the independent and spontaneous creation of local self-defense units, it is not at all clear why the peasants also required a ruling elite. One would imagine that if the peasants could organize themselves into guerrilla units, they could also take care of other village business themselves. Except for a need to coordinate inter-village defense, Johnson nowhere explains why the peasants needed this new ruling class. And if Johnson had described the social structure of the traditional village, he would have had much trouble in justifying the necessity, in terms of peasant warfare, of the elites.

An examination of Chinese rural life would have shown that the village gentry had very little to do with the peasantry except for exacting labor, taxes and rent. Without the simultaneous brutality of the Japanese, the peasants could only have welcomed the disappearance of the rural elite. In traditional China the elites were repressive, performing very few beneficial functions for the peasantry. It may have been true that the peasants needed leadership. But – elite theory to the contrary – leadership is not identical with elitism. The last thing that the Chinese peasantry needed was a new elite simply replacing, in function and purpose, the old elite.

METHODOLOGY, or DID JOHNSON HAVE A CHOICE?

Internal Logic of the Book

Johnson's argument appears to be as follows. He states that there was no substantial peasant support for the CCP before the Japanese invasion. He asserts a great deal of peasant support during the war. It is not clear whether this is an assertion or a thesis to be proved. However, he later dismisses other theories accounting for this rise, so one can take it as a given. He says that he wants to show that the basis of this support was nationalism created by the Japanese invasion. His main defense of this conclusion is the growth in size of the CCP Army and CCP-governed areas in Japanese territory. In fact, most of the book is devoted to military matters. However, all that Johnson has shown is that in areas affected by the Japanese there was an increase in Communist following. One cannot conclude from this on what grounds the support arose. (Nor is it immediately clear what long battle descriptions have to do with the issue.)

Japanese Sources

One could partly explain the peculiarity of the argument by noting that Johnson was limited in scope to Japanese areas and military matters since he relied primarily on Japanese military documents. This leads into a discussion of Johnson's use of source material.

Much of Johnson's work is based on official Japanese military and intelligence documents. He has often been criticized on the grounds that these sources contain a bias toward military matters and hence will stress the war and nationalist activities of the CCP. These documents have also been attacked as biased toward the Japanese and as having a distorted and inaccurate understanding of China, peasant life, the CCP and its organizational efforts.

Johnson tries to anticipate these objections by countering that they can be expected to be accurate accounts since they were employed for "a realistic appraisal of military and political developments." (x) One need only reflect on the problems of using U.S. military and intelligence reports on the Vietnam War in order to have some doubts about Johnson's defense.

Yet he must have known the biases of the Japanese sources when he picked them. Indeed, I would like to suggest that that is precisely why he picked them, and that researchers always exercise judgement and make choices when selecting what references they will use and that these choices are guided by the type of answers they want and the type of questions they ask. Moreover, it is surprising, considering the debate over the use of Japanese sources, to find that the claimed reliance on them is actually overstated. While most of the information on battles and the growth of Red areas and Army is from Japanese sources, most of the details on village organizing come from captured Chinese documents, U.S. Government reports, and Westerners' eyewitness accounts.[38]

References don't pick scholars; researchers actively go out and find what it is that they feel can help them. I am therefore suggesting that criticism of the use of Japanese military sources has to be made simultaneously at a deeper level, that of theory.

Chinese Communist Sources – What Ever Happened to Participant Observation?

What I consider to be a more telling criticism of Johnson is not his supposed selection of one particular type of source material, but rather his non-use of other references. Almost as startling as the inclusion of Japanese military documents is the exclusion of Chinese Communist party material and other Chinese reporting such as local newspapers.

Aside from several incidental references in the footnotes, Johnson never quotes from Mao. And only a few non-captured Chinese Communist documents are used. It is not, as Johnson implies (x) due to a lack of available material. Gillin, for example, has numerous Chinese sources for the same material and the same period.

The many available CCP official documents contain much information about Communist strategy and tactics. *The Selected Works of Mao Tse-tung* contains numerous internal party directives, speeches, theoretical analyses about such subjects as the place of nationalism in the Chinese revolution and during the United Front, and the purpose and tactics for implementation of social and economic programs. The only justifiable reason for not using these is that they are inaccurate, i.e., that Communists lie. But doesn't Johnson's argument that objectivity is necessary for military matters hold for the Chinese Communists as well as for the Japanese? Aren't internal CCP documents as trustworthy as Japanese ones? Since much available Communist material consists of internal party documents, does this mean that they lie to each other? Is everything a diabolical power play by a few

Machiavelians at the top of the party?

This omission of official CCP material is even more odd considering that Johnson relies heavily on Japanese-captured Chinese documents. Does he consider the uncaptured ones less trustworthy, forgeries after the fact? Do the Japanese never "doctor" captured documents?

Western Sources – You Can Trust Some of the People Some of the Time

As glaring as Johnson's ommission of CCP documentation is his misuse of Western observer accounts. Johnson uses sources such as Snow and Taylor for much of his information on actual party work in the villages and the internal workings of the CCP and Red Army. However, he usually cites only these writers' descriptions of observable "things" – army study rooms, methods of training soldiers, government organization, cadre school curriculum.

Yet these books also overflow with analyses of why the peasants supported the Communists, the meaning of various social programs, the importance of nationalism. For example, Belden and Snow, in many conversations with the Chinese, asked them why they supported the Communists, what they thought about the war, the Army, nationalism. As Gillin notes, rarely do they give anti-Japanese feeling as the reason for supporting the CCP or even for joining the army. They also contain many interviews with peasants and Communist officials. Johnson uses little of this. That is, you can trust Edgar Snow to describe what books and pictures he saw in an army club room, but not to tell you what it means. (Johnson, p. 83)

Thus, the sources which Johnson uses to document much of his information on actual party work in the villages contain information contradictory to his thesis. Is it omitted because the peasants lied? If so, why did they always tell the same lies to every Westerner? Were the peasants duped by the Communists or were they too stupid to know why they supported the Communists?

But it is not just the impressions given by peasants that Johnson ignores. He also neglects most interviews with and statements by CCP and government officials.[39] Does Johnson assume that they always lied to Snow and that Snow was consistently dumb enough to believe them and to publish it? In fact, Johnson's reluctance to use what the Chinese themselves said about what they were doing borders on the bizarre.

Elite Theory

This is really the heart of the matter. *Peasant Nationalism and Communist Power* is really a study of what Johnson considers to be a change in elites, not a study of a mass social movement. The book is not an example of applied social movement theory but an illustration of how elites *use* social movements to gain and keep power; i.e., social movements are treated as a means for facilitating the changing of elites. Note that the title states exactly that – the use of peasant nationalism in enabling the Communists to attain power. So one should not expect that the study will be related to the main questions in social movement theory, e.g., type of mass organization, methods of recruitment and retention of members, actual policies and programs. Rather, the questions relate to the changing of elites. Thus the focus of the book is on how the CCP replaced the KMT. There are two aspects to this change – how the KMT lost control and how the CCP gained it.

Elite theory cannot explain change (since elites are always insuring their rule) in structural terms. Elite theory is based on a continuing, stable concept of society. It is not concerned with why elites fail, but with how they retain their power and legitimation. Since there is no real interaction between elites and the masses, it is difficult for the masses to challenge or remove the legitimation of the elites. Because an elite is always reinforcing its power and position and because it is difficult for challenges to its authority to come from within, it is often necessary to introduce an outside force (the deviant or *deus ex machina* theory of social change). Elites don't fall *because* of themselves; rather they fall *in spite of* themselves.

Johnson gives just such a description of the fall of the KMT in China. Johnson claims that the Japanese invasion drove the elites from the villages. The fall of the KMT is presented as due largely to the outside invasion, rather than to structural reasons or to reasons specific to the KMT such as inadequate, unresponsive and repressive policies. Change in China was not due to the elites, according to Johnson, but rather to the absence of elites.

Consequently, there is no need to examine the realities of rural life in China, the harshness and injustice of the old ruling elite and the demands by the peasants for change. And since the actual policies of the old elites are irrelevant, so are the policies of the new elites. It is thus not as surprising as it first appeared that there is very little social, economic or political description of life in China. It really does not matter. One does not evaluate elites in terms of the quality of their rule; one merely notes their comings and goings. Johnson is not only spared describing how the old elites do not meet the needs of the people, he is also spared having to deal with how the Communists did in fact try (often successfully) to confront the real problems of Chinese society.

What is his explanation for the coming of his new CCP elite? He approaches this by asking the two standard elite theory questions: Where did the CCP get its power and how did it acquire legitimation? Johnson answers: the CCP gained power through military victory and legitimation through nationalist ideology.

At last, the oddities of Johnson's argument become understandable. He devotes so much attention to military matters precisely because they constitute the core of his explanation. The somewhat peculiar stress on nationalism is now more clear. Since the main factor in the changing of elites is the Japanese invasion and the anti-Japanese war, obviously the basis of legitimation would have to be the ideology associated with that war. Thus it is elite theory, with change being due to an outside agent – the Japanese – which leads Johnson to his fixation on the role of nationalism.

This also partly explains why Johnson ignores the political, theoretical and social writings of the Communists. They are at best icing on the cake, and at worst irrelevant. The "social movement" aspect of the Communist victory is merely a means for replacing elites, it is not essential for understanding the process.

Elite Theory and Social Movements

So it is not the peasant movement which is of interest to Johnson. It is the CCP leadership (i.e., elite), the movement

being of interest only insofar as it enables the new elite to take and hold power. Thus Johnson approaches the movement from two aspects. How did it enable the Communists to attain power through military means? How did it give legitimacy to the Communist elite through nationalist ideology?

Because of this, Johnson does not deal with areas that are usually an essential part of any analysis of a social movement. First, he never touches on internal movement matters such as structure, recruitment and retention of members, and adaptation to changing conditions. Second, the interaction between the leadership and the base is viewed as one-way and treated superficially. That is, the interest is in how the elite gets the ideological support of the base. The dynamic of the interaction between the base and the leadership is ignored. Also, this giving of support is viewed as a one-time, static occurrence – elites *get* support and after this support is established fact, they hold it usually on the basis of the original reason regardless of new circumstances. Lastly, the actual things that the movement does, economic and social programs, are largely irrelevant.

In elite theory, gaining and holding power becomes the prime goal, not a means for other ends. Johnson has taken the obvious fact that political movements want state power and elevated it to the position of being the main explanatory insight. What is more, it becomes a means to criticize the Communists. Johnson ignores the goals of the movement and then accuses the movement of having no goals except gaining power. (To see the silliness of this rather common line of reasoning, consider accusing the Democratic party of being power-mad because it wanted to win elections.)

Related to Johnson's considering power as an end and not asking what the power was to be used for, is the lack of concern over the form and distribution of power. He sees the CCP as simply replacing the rural elite. He does not consider any altering of the distribution of power in the villages. Nor does he consider the creation of new institutions through which power can be exercised. This omission is due to the inability of elite theory to deal with changes in the social structure.

Along with gaining power, the other main use of a social movement for elites is legitimation. In this respect it is not as important what a movement does as what a movement says. And the ideology is not only important for gaining power, it is even more important as the basis of later power.

> *The actual source of the Communist Party's authority in China today dates from the wartime period when it led the mobilized masses of previously non-Communist areas in their struggles with the Japanese Army. (49)*

It also follows from Johnson's view of elites and social movements that the base (mass) is necessarily coopted and manipulated by the leadership (elite). Implicit in Johnson's analysis based around the concepts of power and legitimacy is the conclusion that the CCP used the needs and demands of the peasantry to advance their own ends.

In Johnson's presentation nationalism and anti-Japanese local defense leagues grew spontaneously. The CCP spread, coordinated and profited from this movement, but it did not create it. The picture given is of an elite comandeering a social movement. (9-10) Not only did the Communists coopt the peasants by using their protest against the Japanese to gain state power, the Communists were also manipulative in that once they had legitimacy based on the anti-Japanese war, they introduced social and economic programs, i.e., socialism.

Because elite theorists have no concept of leaders being structurally responsible to their base and because they consider elites to act primarily out of a personal drive for power, it is impossible (other than in some moral sense) for elites not to coopt and manipulate the masses. If elites meet the needs of the base, they can only be doing so in order to gain power and legitimation. If elites add additional demands and actions, then these can only be seen as imposed upon the masses, since the base is still supporting the elite for the initial reason. Elites, in other words, always hold power at the expense of the mass.

Johnson does not put the Communists' anti-Japanese policy into a broader strategic context (even though they do), because he does not consider the total ideological framework of the CCP. He gives no analysis of peasant life and peasant needs. And, because elite theory does not conceive of any interaction between the leadership and the base, he conceives of the relationship between the leadership and the base as one of the base giving consent to elite rule. That is why Johnson is reduced to giving explanations in terms of the feelings of the masses.

Johnson's lack of a social, historical, or economic perspective is not accidental. It stems from his view of what constitutes social forces and social needs. Johnson is an elitist; that is, he views history as an endless flow of ruling elites continually replacing each other in colorful and exciting social crises. In order to understand any particular replacement, all one needs to discover is how the new elite managed to gain power; and therefore in order to understand history, all one needs to discover is how the old elite lost its authority, how the new elite managed to gain the support of the mass (that is, how it got its legitimacy), and how the new elite got power.

Notes

1. Donald Gillin, "Peasant Nationalism in the History of Chinese Communist," *Journal of Asian Studies*, 23 (February 1964), 289.

2. I go into this in the methodology section when I discuss Johnson's use of resource material. Gillin in his review has a very nice example. In footnote 51 on page 276, Gillin notes how Johnson either did not read carefully one of his sources or else ignored the information because it contradicted his hypotheses.

3. Ilpyong Kim, "Mass Mobilization Policies and Techniques Developed in the Period of the Chinese Soviet Republic," in *Chinese Communist Politics in Action*, ed. A. Doak Barnett (Seattle: Univ. of Washington Press, 1969), 79.

4. O. Edmund Clubb, *Twentieth Century China* (New York: Columbia University Press, 1964), 201.

5. Roy Mark Hofheinz, "The Ecology of Chinese Communist Success: Rural Influence Patterns, 1923-45," in *Chinese Communist Politics in Action*, 48.

6. Kim, 78.

7. Kim, 98.

8. For a discussion of this see the chapter on the village in Franz Schurmann, *Ideology and Organization in Communist China* (Berkeley: Univ. of California Press, 1970). Mark Selden also goes into this in the first few chapters of his thesis, *Yenan Communism· Revolution in the Shensi-Kansu-Ninghsia Border Region, 1927-1945*. Doctoral dissertation, Yale University, 1968.

9. Mark Selden, *The Yenan Way in Revolutionary China* (Cambridge: Harvard University Press, 1971), 92.

10. Selden thesis, 32.

11. Ibid., 37.

12. Ibid., 38.

13. Ibid., 43.

14. Ibid., 46.
15. Ibid., 47.
16. Ibid., 54.
17. Gillin, 269-70.
18. Ibid., 271.
19. Selden thesis, 263-274.
20. Some of the relevant selections in volume II are "The Chinese Revolution and the Chinese Communist Party" and "On New Democracy." One can also see in volume III, for example, "Spread the Campaigns to Reduce Rent, Increase Production and 'Support the Government and Cherish the People' in the Base Areas" and "Economic and Financial Problems in the Anti-Japanese War" and "Get Organized!"
21. Kuo-chun Chao, *Agrarian Policy of the Chinese Communist Party, 1921-1959* (New Delhi: Asia Publishing House, 1960).
22. Mao Tse-tung, *Selected Works* (Peking: Foreign Languages Press, 1965), II, 93.
23. In his thesis, Selden discusses the problems in keeping up the revolutionary interest of the peasants after they had received land from land reform programs.
24. Gillin, 284-5.
25. Ibid., 282.
26. Ibid., 283-4.
27. Ibid., 285.
28. Since Selden's thesis has been published as a paperback book, *The Yenan Way in Revolutionary China*, I will not go into any of these programs in detail.
29. The analysis of the election campaigns to follow is from my M.A. thesis, *Rural Revolution. Organizational Activities of the Chinese Communist Party in the North China Countryside, 1935-1949.* (Humboldt State University, California, 1972)
30. William Hinton, *Fanshen* (New York: Monthly Review Press, 1966). Li-po Chao, *The Hurricane* (Peking: Foreign Languages Press, 1955).
31. The recent movie "The Sorrow and the Pity" comes to mind. In this movie it is very clear that the resistance activities of the French peasants were not popular with their village friends and neighbors. One can only wonder if some of the same feelings existed in China.
32. Edgar Snow, *Red Star Over China* (New York: Grove Press, 1968), 241-45.
33. Gillin, 281.
34. Gillin, 281.
35. Selden thesis, 101.
36. Selden, *Yenan Way*, 119.
37. In short, I am suggesting that Johnson would make a lousy theorist of social change and revolution. His book *Revolutionary Change* (Boston: Little, Brown and Company, 1966) bears this out. The heart of his theory is that in times of stress and potential revolution sometimes something critical happens and then a revolution occurs. Just as in his book on the Chinese revolution, he has no way of predicting or really explaining, in structural terms, why this should happen. He is essentially saying that if it happens, it had to.
38. For example, in his discussion of peasant activities (97, 100) he uses one Westerner, one Chinese Communist writer, and some Japanese-captured CCP documents. Similarly in such a description on pp. 99-100, he uses two Westerners, one U.S. Government document, and one Japanese source.
39. For example, see the interviews in the chapters "Life Begins at Fifty," "Why is a Red?", and "Session in Politics" in *Red Star over China.*

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The Sino-Soviet Boundary Question

by Neville Maxwell

The Sino-Soviet Territorial Dispute, by Tai Sung An. Philadelphia: Westminster Press. 163 pp, 47 pp documentary appendices, $8.95.

The problem of the boundaries between the People's Republic of China and the Soviet Union was very nearly solved by a single decision of the new Chinese government, taken soon after it assumed power in 1949.* The Communists' predecessors, Nationalist and imperial, had made clear their determination, no less implacable for its being fruitless, to regain the territory that had been taken from China during the long prostration that followed the Opium War (1840-42). The Japanese, the French, the British had all pressed hard on China and trimmed off areas which the Chinese regarded as their own; but Tsarist Russia had amputated huge tracts of territory from China, and these were the lost lands that dominated the irredentist dreams of the Nationalist (and nationalist) Chinese. China's new men reversed their predecessors' policy. Choosing to concern themselves with the future of China as history had left her rather than with attempts to restore the boundaries of her imperial past, they turned their backs on the bequeathed irredentist claims – and, they must have expected, on the intractable disputes which such claims must inescapably have entailed. The People's Republic of China has repeatedly declared that it makes no territorial claims on any neighbor.

But the decision to abjure territorial claims did not in itself solve the problem of the boundaries. That remained, and was to bring China to war with one of her neighbors and to what seems a perennial confrontation with another.

The problem was that many sectors of China's vastly long frontiers had not been submitted to the process of boundary settlement which is required to produce the only sort of boundary a modern state can live with – a defined, linear boundary. (In the pre-modern period precision was rarely called for, and perhaps the most comfortable boundary was one of separation by a no-man's land.) Some sectors of China's frontiers had not been delimited at all, most conspicuously the long southern stretch between herself and India, Pakistan and Nepal; others had been delimited but not demarcated,[1] or delimited on the basis of such primitive maps or surveys that the boundary remained uncertain and disputed. At the Bandung conference in 1955 Chou En-lai stated his government's readiness to acknowledge that parts of China's borders were undetermined (seeing that to insist that they *were* determined would be tantamount to trying to impose a unilateral settlement on China's terms) and to proceed to the determination of boundaries, where necessary, by due diplomatic process. "We shall use only peaceful means," he said, "and we shall not permit any other kind of methods. . . ." In 1959, in the course of its exchanges with India, the Chinese government elaborated and emphasized its approach to boundary questions:

> *So far as the question of boundaries is concerned, China absolutely does not wish one inch of another's territory. There are undelimited borders between China and many of its neighboring countries, but China has never taken, and will never take, advantage of this situation to make any change in the actually existing state of affairs on the border by unilateral action. Whether or not the boundary has been delimited, China is always prepared to work in close cooperation with its neighbors . . . so that there will be no mutual misgivings or clashes over the border question.*

The record of China's dealings with her neighbors demonstrates scrupulous adherence to that undertaking, and this approach has earned China the high benefit of mutually satisfactory settlements with seven of her neighbors – Burma, Nepal, Pakistan, Afghanistan, Mongolia, Vietnam and Korea.[2]

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* This review appeared first in *Le Monde Diplomatique*, March 1974, in French.

1. The distinction between these two terms is crucial to clarity in any discussion of boundaries. (It is ignored in the book under review.) The distinction was coined by Henry McMahon, who was to give his name to the boundary alignment claimed by India in her northeast: as he put it, "delimitation" comprises the joint determination of a boundary line by agreement between the governments concerned, and its definition in verbal terms; while "demarcation," the subsequent step, comprises the actual laying down of a boundary on the ground and its definition by pillars or other markers.

(That only leaves Laos: and in that instance, the writer learned in Peking, the boundary is regarded by both sides as already settled.)

Various as their boundary problems were, the Chinese evolved a procedure which covered them all. The first step in this would seem truistic, but is far from that: agreement between the neighbors that a boundary dispute exists, and the joint definition of which areas are in dispute. Next, there follows agreement to maintain the status quo on the ground, and thus to preserve both sides from the temptation to press forward into disputed territory, with all its attendant risks of clashes. And finally, if clashes have already occurred, or if the presence of both sides in a disputed area creates a danger of clashes, then both sides agree to withdraw armed forces from disputed areas, or for an agreed distance from the line of actual control. When the situation on the frontier has been frozen and defused by the application of that procedure, negotiation can proceed without strain and in due course be followed by the establishment of a joint commission to demarcate the agreed boundary. That approach was judged to be fair and practical by most of China's neighbors, and their governments have settled their boundaries with China; but her two biggest neighbors, India and the Soviet Union, refused to adopt it.[3]

Once the Chinese had decided to write off all the old irredentist claims to areas annexed by Russia in the 19th century, the problem of the Sino-Soviet boundaries became in itself quite insubstantial: Premier Chou En-lai observed in 1960 that it amounted to "only a very small discrepancy on maps and is very easy to settle." The full stretch of the boundaries, more than 7,000 kilometers, was delimited under the 19th century treaties: those treaties were, in the eyes of the Chinese, "unequal" (that is, imposed) – and indeed it is primarily Soviet historians who have attempted to dispute that, and they only recently – but the Chinese government has declared that it is prepared to respect those treaties; and it gives cartographic expression to that acceptance in every official map. Small-scale maps showing the whole of China on sale in China (and pinned in houses and halls all over the country) show only one very short stretch of the Sino-Soviet border, in the Pamirs, as requiring further definition; all the rest is marked as a full, final international boundary. The other areas in dispute are so marginal as to show only on large-scale maps; and of course the dispute over the border rivers in the east might be described as a matter of only a few hundred yards – except that the short distance makes the difference between equal rights on those great navigable streams and exclusivity for the Russians.

That dispute over the rivers is one of the differences which has emerged in interpretation and implementation of the boundary treaties, and which makes re-negotiation necessary. The Russians claim that the treaties which made the Amur (Heilung) and Ussuri (Wusuli) Rivers the eastern boundary gave the entire breadth of the rivers to them by placing the actual line of division where the water meets the Chinese bank. There is no allusion to any such provision in the text of the treaties; and consequently the Chinese reject the Russian claim and maintain that according to the principles and practice of international law, the line of actual division in the rivers follows the *thalweg*, a line along the deepest part of the main channel.

The Soviet claim rests on an exchange of maps between Russian and Chinese boundary commissioners in 1861. This was meant to depict and confirm the provisions of the Treaty of Peking, signed in the previous year; but P. Kozakevich, on the Russian side, apparently took advantage of the inexperience of the Manchu mandarins he was dealing with to gain a little extra for his tsar. He drew the boundary not through the Amur/Ussuri confluence, where the treaty puts it, but on a shallow stream connecting the two rivers some 40 kilometers upstream from their confluence. That channel creates an island, called by the Chinese Hei-Hsia-Tzu ("black-blind," local parlance for Bear Island); and Kozakevich's maps put the island on the Russian side of the boundary.[3a] The Russians named the channel after Kozakevich, on whose cartographic sleight-of-hand the Soviet Government also bases its claim to a boundary along the Chinese bank.

The Soviet Union has stretches of river boundaries with several other neighbors and now in every other case accepts the *thalweg* principle, which provides for equal rights to navigation and other use:[4] that the Russians reject this principle in the case of China (and in 1969 fought – and apparently lost[5] – two hot battles over a tiny island in the Ussuri to sustain their claim to exclusive rights on the rivers) probably reflects Soviet determination to keep possession of Hei-Hsia-Tzu, just across the water from the city of Khabarovsk. Application of the *thalweg* principle would make that island China's. The island, as territory, is unimportant, but it seems that the Russians, who hold it, are determined not to have the Chinese on the other side of the river from Khabarovsk. This is also a sticking point for the Chinese because the Russians have used their occupation of Hei-Hsia-Tzu to prevent Chinese river-boats from plying through the confluence from one river to another; and the Chinese are absolute too in insisting on the *thalweg* principle as a matter of legal and historical right, not as a Soviet concession (there have been indications that the Russians would concede the *thalweg* principle in return for Chinese relinquishment of their treaty right to the confluence island).

Another element of the Sino-Soviet boundary problem concerns areas which Tsarist Russia occupied after the treaties

2. The boundary settlements with Vietnam and Korea have not been made public. But the PRC has stated that it has "settled ... boundary questions ... with all its fraternal socialist neighbors except the Soviet Union. ..." Letter from the CCP to the CPSU, February 29, 1964 (*Peking Review*, May 8, 1964).

3. For the Indian approach to the problem of the Sino-Indian boundary see the reviewer's *India's China War*, published in paperback by Doubleday-Anchor in 1973.

3a. It may be noted that some authoritative atlases, such as those of *The* (London) *Times*, depict the boundary at the confluence on the alignment claimed by the Russians; others follow the Chinese version–and the latter include the atlas of China prepared by the C.I.A. for Nixon's Peking visit and later published commercially by Rand McNally.

4. Until Stalin agreed in 1946 to renegotiate the Soviet-Afghan boundary, the Soviet Union had a treaty right to exclusive possession and use of the Oxus where it made the boundary, and Afghans attempting to use the river were sometimes shot by Soviet guards. (Parallels between the Sino-Soviet and Soviet-Afghan river disputes were brought to the reviewer's attention by Dr. George Ginsburgs.)

5. See the reviewer's "Report from Chenpao Island" in *China Quarterly*, November-December 1973.

were concluded, or which the Soviet Union has moved into in recent decades, *in excess* of their treaty entitlement. An example from Tsarist days is a patch of territory between the Amur and its tributary the Zeya, on the Russian side (now adjoining Blagoveshchensk) which the relevant treaty left as a Chinese enclave but which the Russians annexed in 1900). But China does not lay claim to this tract; and in the early 1950s, reversing the cartographic practice of its predecessors, the PRC stopped showing it as Chinese territory – to the indignation of the KMT in Taipei. There are other areas which the Russians occupy and would certainly refuse to relinquish, although strict application of the treaties would render Soviet possession of them adverse. The Chinese approach to that problem has been clearly stated: "Any side which occupies the territory of the other side in violation of treaties must, in principle, return it unconditionally to the other side, but necessary adjustments may be made by the two sides in accordance with the principles of consultation on an equal footing, and of mutual understanding . . . and in consideration of the interests of local inhabitants." What that means in practice has been shown in China's successful boundary settlements, for example with Burma. In that case Burma held an area, important to her, on lease from China (an arrangement dating from the days of the British and the Manchus): in negotiating the Sino-Burmese boundary the Chinese ceded that territory to Burma, in exchange for another tract, of equivalent area, which was of no particular importance to the Burmese. The Chinese approach to the Sino-Soviet boundary problem, as to all boundary questions, might be described in the words of a 19th century British official concerned with boundary-making in India: ". . . it is not a strip more or less of barren or even of productive territory that we want, but a clear and well-defined boundary." The Chinese are ready to settle their boundary with the Soviet Union, but they will do so, as they have put it, only "in the spirit of consultation on an equal footing and mutual understanding and mutual accommodation." In their view, the Soviet approach to the boundary question expresses the contrary spirit and is an attempt to impose a settlement on the sole principle of the political and strategic requirements of the Soviet state.

The basic Soviet approach is kindred to that so self-destructively adopted by the Indian government. The Russians maintain that there are no disputes over the Sino-Soviet boundaries.[6] Moscow suggests that, by introducing the "concept of disputed areas," the P.R.C. is "seeking to gain unilateral benefits and advantages before the opening of negotiations, without any consideration for the national interest of the other side," as a Polish journal recently put it.[6a] The opposite would also apply. That is, by rejecting the "concept of disputed areas," Moscow is attempting to make its own reading of the treaties a legal absolute, and thus "seeking to gain unilateral benefits and advantages before the opening of negotiations . . ." The territory which the Chinese describe as disputed (because it is claimed by both sides) the Russians maintain is unquestionably their own: theirs, if not by right of treaty, then by right of what they call historical development; and in instances where their occupation of territory is too recent to sustain description as "historical," then by right of actual possession. The Soviet position is that the boundaries are already "clearly and precisely determined by treaties, protocols and maps," and they have been chary of entering into the comprehensive negotiations sought by China. The Russians originally stated that they were prepared only to "discuss the question of specifying the frontier lines over individual stretches," with the clear implication that Moscow should have the say over which stretches required "specifying." And while themselves bringing the strongest military pressures to bear against China, the Soviet government has persistently presented itself as threatened by China and misrepresented the Chinese approach to the boundaries as one designed to seize for China the vast areas of the Soviet Far East.

A careful and informed reader can pick out that factual outline in Dr. Tai Sung An's book, but all too often it is distorted or obscured by the author's preconceptions. Dr. An's premise is that the People's Republic of China, whatever it may do or say, is recognizably heir to the irredentist hankerings of former Chinese governments. He includes in his short study the evidence which refutes that view, but takes little account of that evidence himself; while the evidence he advances to substantiate his premise is skimpy or – to put it politely – mistaken. For example, Dr. An says that "Shortly after 1949 . . . Chinese dissatisfactions with the present Sino-Soviet frontiers in Asia were manifested in many directions." As if to illustrate that point, he goes on: "Numerous maps published in China under Communist rule provided evidence that the Chinese Communists had not abandoned China's traditional interest in the 'lost territories' in Asia." The cited source for this statement is a geography of China written in the mid-1950s; but if the reader is wary enough to follow up that reference, he finds that it gives no substantiation whatever to Dr. An's statement. The passage cited notes only that in the mid-1950s (before China had begun boundary negotiations with any of her neighbors) official maps in China showed various sectors of the boundary as "undelimited" – which at that time they were.

The fact – all too often ignored by China-watchers like Dr. An – is that all contemporary maps published in China show the boundaries with the Soviet Union on the alignments the treaties with Tsarist Russia gave them. Western writers on Chinese affairs (Dr. An, a Korean by birth, is a naturalized American) have derived almost incredible mileage from one history book published in Peking in 1955, which contained a map illustrating the furthest limits of the old Chinese empire, described as doing just that. There was nothing in the book – or anywhere else – to suggest that the map was meant to show anything but the past, but it is again and again cited as if it were a blue-print, covert or perhaps inadvertent, for revanchist reconstruction of imperial China's domains. Dr. An duly trots out this sorry exhibit. He notes that the Chinese "contended" that the map was unofficial and even that "China officially

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6. This stand may sound nonsensical, but Jawaharlal Nehru, who took exactly the same position, advanced an explanation of sorts: "According to our thinking our trouble at the border is not a dispute with China. It is a question of words, perhaps. It is a dispute, of course –'when we argue about something, it is a dispute. But my point is, it is not a dispute because we have no doubt about our own position in this matter. So far as we are concerned, we are clear that this is not a normal dispute but it is just a claim on our territory which is ours, and we are convinced that it is ours." (In the Rajya Sabha, the upper house of the Indian parliament, 20 February 1961)

6a. TASS, Warsaw, June 25, 1974, quoting *Zycie Warszawy.*

disassociated itself" from the map; but he still cites it as evidence of "Chinese dissatisfactions with the present Sino-Soviet frontiers in Asia." The map continues to circulate, he points out, without indicating where: it does not appear to be available in Peking.

Analysis of any diplomatic exchanges demands the closest attention to exact wording, and of course scrupulous fidelity in quotation. Dr. An is culpably remiss in both regards. For example, in describing Peking's stand on the question of territory held by the Soviet Union in excess of its treaty entitlement, he indirectly quotes the passage given earlier in this review – and omits the crucial phrase, "in principle." That omission enables him to state, misleadingly, that "Peking demands that such additional lands be returned to the Chinese people unconditionally." Again, China, trying desperately to make itself heard, it sometimes seems, over the inner voices to which her critics prefer to listen, stated in 1969 in words, so to speak, of one syllable: "China does not demand the return of Chinese territory which Tsarist Russia annexed by means of [the 19th century,] treaties." Again Dr. An paraphrases instead of quoting, and again he misrepresents, this time by insertion instead of omission. In his version, "China did not demand the outright return of her territory that Czarist Russia had annexed" – implying by the inserted word, "outright," that in some roundabout or delayed way China does mean to get that territory back. These are not quibbles. Any lawyer or diplomat would be likely to see these phrasings as stitches in a pattern.

As a whole, Dr. An's study is unsatisfactory, being misleading for students of the subject and inadequate for specialists, confusing rather than clarifying the issues. There are, however, useful sections in it, and not only the appendices giving texts (some only partial) of the boundary agreements, treaties and related documents. His chapter on the historical background is lucid and comprehensive; and his chapter, "Sino-Soviet War in the 1970s?," succinctly summarizes what has been written by the disputants and opined by outsiders about the likelihood of Sino-Soviet war. Dr. An's own answer to his question about war in the 1970s is in the negative. He concludes that the balance of terror will come into effect between the Soviet Union and China as it did between the Soviet Union and the United States. The Chinese would have agreed with him in 1973 so far as immediate prospects were concerned, but they – and outside observers – are concerned about Soviet military preparation which may suggest intention to attack in 1974/75. Anyway, they would be categorical in saying that the risk of war – that is, of attack by the Soviet Union – will continue for a very long time.

Since the Soviet army stopped shelling the Ussuri island Chenpao/Damansky shortly after the meeting between Chou En-lai and Kosygin in September 1969, there have been no more confirmed clashes on the borders.[7] They continue tense, however, with dangerous criss-crossing of patrol-routes in the west and reported incidents of harassment of Chinese use of the eastern border rivers by Soviet gun-boats. The negotiations that have been going on in Peking sporadically since 1969 are deadlocked. The Soviet troop and missile concentrations on the Sino-Soviet and Sino-Mongolian borders have not been reduced since their build-up in 1969, indeed the concentrations have been increased; and the Chinese continue braced to resist attack.

Until recently at least, the Russians, in Peking and in other capitals, were energetic in pooh-poohing the thought that they would ever dream of attacking China. Is Chinese fear of such an attack a figment of "Russophobia," as Dr. An calls it? Well, it cannot be forgotten that in 1969 the Soviet Union not only gave every indication of deadly preparation to launch an attack, but even sounded out the American administration (as well as fraternal communist parties) to solicit approval or at least understanding for a war on China. (It seems merciful that the Russians did not get to that point during the Kennedy administration. It is reported that President Kennedy "ordered exploration of the idea of destroying [China's nuclear capacity] in some sort of collaboration with the Kremlin"[8] – and certainly that course was being advocated in the early 1960s by reputable American commentators.)

The evidence in 1969 was enough to convince Washington that the Russians were on the point of attacking China, and the huge Soviet military concentrations in positions of strategic attack on China's borders are still such as to make Washington conclude that the odds on a future Soviet "preventive" attack are even.[9] So it is hardly surprising that the Chinese have in recent years exerted an immense effort to prepare themselves to resist. The Chinese do not play Russian roulette.

The Chinese declare that the ideological differences between themselves and the Soviet Union are irreconcilable, and if the present directions of political development in the two countries continue unchanged, this must remain the case – indeed, if the Soviet Union's growth into a state-capitalist version of the consumer society accelerates, the gap must widen. But the Chinese also argue that these ideological differences need not prevent the maintenance of normal state relations between the two countries – and certainly need not prevent the settlement of the boundary question.

They make the same sort of basic distinction between ideological issues and practical considerations of state relations in the matter of the "unequalness" of the 19th century boundary treaties, argument over which has become such a loud and obsessive chord in the Sino-Soviet polemics. The Chinese will go on arguing this for ever, and so, it seems, will the Russians – who are turning out a new historical literature to substantiate their argument that the treaties were not unequal. But this is a parallel issue. The Chinese are *not* making a Russian admission that the treaties were unequal a precondition for progress to settlement of the boundary question.[10] If the boundary question itself could be settled, and the settlement expressed in a new treaty, argument over

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7. Moscow reports of a serious incident in the western sectors in 1973 were denied by Peking.

8. Joseph Alsop, "Go Versus No-Go," *New York Times Magazine*, March 11, 1973.

9. Ibid. "Preventive" here can be read as distinct from "pre-emptive." Even the Russians can hardly be serious in suggesting that China is preparing to attack them. What is sought to be "prevented" by what Mr. Alsop called "the nuclear castration of China" is China's continued development into a great, socialist power.

10. This represents a change, or at least a clarification, of the position stated in the Chinese government's document of October 8, 1969, *Refutation of the Soviet Government's Statement of June 13, 1969.* But that the question of the historical nature of the treaties, although in the Chinese view one of high principle, is a side-issue so far as a boundary settlement is concerned can be stated on the highest Chinese authority, and is confirmed by the Soviet side.

the nature of the superseded treaties would become wholly academic.

The deadlock in the negotiations in Peking has developed over the implementation of the understanding reached between Kosygin and Chou En-lai when they met at Peking airport in 1969. The Russian side, it appears, as well as refusing to concede the existence of disputes over the boundaries, is declining to enter into any formal agreement to maintain the status quo and does not accept the Chinese proposal for reciprocal withdrawal of armed forces from disputed areas. As was disclosed by Moscow, the Russians did suggest a no-war pact and/or an agreement not to use nuclear weapons. The Chinese rejected that proposal on the grounds that if the Russians were unwilling to take the practical steps that would reduce the danger of war, any verbal gestures in that direction would be suspect, if not worthless. The Chinese experience with treaties and agreements with the Soviet Union has left them skeptical: they do not forget Krushchev's action in tearing up all the agreements his government had signed to assist in China's economic development and withdrawing Russian aid technicians on a few weeks' notice.

Thus the boundary question, which the Chinese once believed to be "very easy to settle," has become insolvable short of reversal by one of the two governments. It has become enmeshed in the great falling-out between the Soviet Union and China – or rather, in the reversion to the historic attitudes of conflict and distrust that quickly followed the false dawn of socialist amity which marked the 1950s. The accident of imperial history that made the Russians China's neighbors perhaps made for an irrepressible conflict – and the changing power equations of the present are bringing that to a head. The factors that have put a Soviet attack on China in the cards are, essentially, power and fear. The fear is, paradoxically, on the at-present stronger side, that of the Soviets, and is largely self-induced. Mis-identifying the Chinese with the Mongol hordes who ravaged Russia (the Chinese were in fact fellow-victims of the Mongols), the Russians appear to have whipped themselves into a racial-historical complex about China. It seems that the Soviet government, while obviously using those fears for its own purposes, may have convinced itself that the Chinese government is as Russian propaganda paints it, bellicose, expansionist and irrational; and that such a government will inevitably, when it has the strength, take up again the implacable irredentism of the past and set about acquiring the Soviet Far Eastern territories.

But, on another level, it is the slow but steady tilt of power between Moscow and Peking as China becomes stronger that provides the explosive content of the Sino-Soviet confrontation. "There can be no genuine third force in the world scene in our times," a leading Soviet commentator observed in 1969: "There is no room for one any more. Those who try to become such a third force have come too late in history." But China's long march to a position of leadership in the world is quickening, and the Russians may feel that only by force might they arrest or reverse a trend which they find intolerable.

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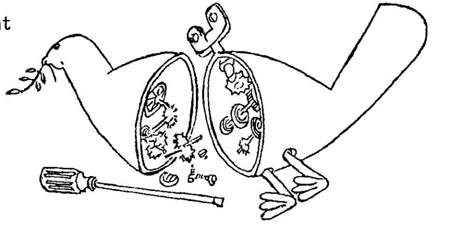

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Harvard on China II: Logic, Evidence, and Ideology

by Cheryl Payer

The Chinese Peasant Economy: Agricultural Development in Hopei and Shantung, 1890-1949, by Ramon H. Myers. Cambridge, Mass.: Harvard University Press, 1970.

Foreign Investment and Economic Development in China 1840-1937, by Chi-ming Hou. Cambridge, Mass.: Harvard University Press, 1965.

The December 1972 issue of this *Bulletin* contained two contributions to an ongoing debate on imperialism in Chinese history from Andrew Nathan and Joseph Esherick. This review article has the more restricted, if not more modest, aim of examining two of the most significant works cited by Nathan and Esherick, both of which have been published in the Harvard University Press's East Asian Series. The two books provide a reinforcing and complementary picture of the effects of foreign economic penetration on, respectively, the peasantry and the indigenous industrial and handicraft production of China over roughly the same time period. I will restrict myself to a critique of the internal consistency and the standards of logic and evidence in the two volumes.

A close examination of sources and their interpretation are crucial for an evaluation of Myers' work. Roughly one-third of the book consists of descriptions of four North China villages which have been compiled from a mass of data gathered by researchers of the Japanese-operated South Manchurian Railway Company between 1939 and 1943. This research project was intended "to shed light on Chinese society in order that its legal and economic system be revamped and a comprehensive plan formulated for Japan to follow with respect to her China policy." (p. 28)

Myers is frank about the potential limitations of material:

> *It was collected by citizens of a conquering power, by foreigners whose compatriots had committed aggression of the most naked kind (p. 37).*

But, having admitted the difficulty he immediately brushes it aside:

> *I have proceeded in the faith – and in the last resort it can be no more than faith – that, despite this, the material is veracious. It seems to be internally consistent. There is no doubt that the scholars collecting it were true scholars, concerned only with the truth and guided at every step by strict canons of scientific method. The real problem is whether the peasants told the truth.*
>
> *Their characteristic tolerance (and long experience) of alien powers suggests that they probably did; nor is it inconceivable to suppose that they could understand that these particular investigators were not the usual servants of the invading army; and the consistency of the evidence they gave suggests this – they could scarcely have planned a consistent set of lies even if they had a devious interest in doing so. [loc. cit.]*

This is disingenuous and evades the real difficulties. It is not necessary for informants to "plan a set of lies" if the same forces are at work on most of them to cause fear, mistrust, or distortion of memory. And although Myers discounts it, there is plenty of evidence in his account that some such forces might have been at work.

"Chinese guerrilla activity made it unsafe for Japanese to venture far into the countryside and for this reason the villages were selected close to the railway lines and county seats." (p. 31) One of the reasons the specific villages were chosen was that "these villages were surveyed before peasants became hostile to the Japanese researchers" (p. 38), but eventually the deteriorating military situation put a stop to field visits even to these villages. We have, in other words, something analogous to a study of Vietnamese village life conducted by researchers employed by USAID in such villages as they can safely reach, and return from, by daylight.

Another serious defect is that the reader of Myers' book is never told precisely *who* in the villages had been selected for interviews. It appears that in at least one case information was collected from county officials. The village headman was invariably interviewed. Aside from that, we are told only that "peasants" were questioned individually. We are not told how many were interviewed in each village, or what the principal of selection was, or how much land they owned, individually or as an average or mean. Yet such information is obviously significant in an area which was so close to the activities of the

Communists and their supporters. Throughout his book Myers refuses to distinguish between rich, middle, and poor peasants – preferring to discuss "peasants" as an undifferentiated mass when he generalizes about rural society. I will return to this point.

Myers claims that these interviews provide a "historical perspective of change in village society" over the last "twenty or fifty" years, and on this claim pivot many of his assertions about the effects of the treaty-port economy and the introduction of cash crops over time. Yet this evidence is based on the memories of peasants whose average age must have been well under fifty, i.e. it was necessarily based on what they had been told by parents and other elders as well as on personal memory.

All these conditions provide grounds for serious reservations, but it is nevertheless true, as Myers claims, that information on rural conditions in China for the period covered is so scarce as to make any source valuable. The evidence need not be thrown out even though it was gathered through interpreters, by government employees of an occupying power, and most likely with interviewing methods which would appal modern survey researchers. The account of the genesis of this project on pp. 27-31 raises, however, even more serious questions about the competence of the researchers to gather data and of Myers to interpret it.

The research project was planned as a cooperative venture between the North China Economic Research Office of the South Manchurian Railway Company (in Peking) and the East Asia Research Office (in Tokyo). A law professor named Suehiro in Tokyo was assigned leadership of the group surveying rural customs. Myers devotes a full paragraph to summarizing an essay by Suehiro which appeared with the preliminary report of the survey, which Myers sees as "a remarkable essay, appearing as it did in wartime Japan in an atmosphere of strict censorship and control, by a member of an academic community generally given to Marxist theory." Please note that it is the *community*, not Suehiro himself, which (according to Myers) is given to Marxist theory. Apart from this very misleading sentence, I find little remarkable in the essay as summarized by Myers, which is about nothing more than the significance of the use of hypotheses for designing and testing research in the social sciences.

What is remarkable is the fact that despite this philosophy of research, "no hypotheses were ever formulated" (p. 29) and the material was collected "on strictly Baconian principles" (p. 37), and that Myers, despite the attention he focuses on Suehiro's essay, does not even seem to perceive the contradiction! He even states that the leader of the field research in Peking, Suginohara, "seems . . . to have unswervingly adopted [Suehiro's] methodological suggestions," although from the information given it seems that he ignored them.

An even more significant point about the research design is buried in a note at the back of the book, which refers to severe criticism of the research proposal from other members of the South Manchurian Railway Company:

> *It is not surprising that the outline received a cold shoulder from many members of the Mantetsu Chōsabu. Judging from the type of village field surveys that had been conducted in north China in the 1930s, it had become axiomatic that the basic departure point was an understanding of land distribution. Once the social and economic classes of a village were defined, to be determined by land ownership and renting land, it was believed that rural conditions would be better understood. Suginobara's outline was simply a means to gather information and group it. This inductive approach must have disturbed and annoyed the majority of researchers, who were convinced that land distribution was the key to understanding village society. Suehiro's legalist approach won out, probably because of the unusual administrative power he held in the Tōa kenkyūjo. The various group rivalries in the Mantetsu chōsabu have never been discussed, but they most certainly existed. [note 16, p. 328].*

In this casual fashion we discover (1) that the research design was contrary to the advice of a significant group of other researchers in its sponsoring agency; and (2) that it was criticized because it turned a blind eye to questions of land distribution – a subject which is, however, absolutely central to Myers' conclusions which are supposedly based on that survey.

It is not surprising that Myers' portraits of the four villages, drawn from data gathered in this manner, are all but worthless for proving or disproving any kind of hypothesis. (Myers admits this implicitly by confining this material to only one-third of the book and drawing upon other sources for the major part.) In these chapters we have the worst of both methods: Myers has abstracted the information so that the reader is not able to judge whether the raw data does in fact support the generalizations, but he has not systematized or analyzed the data sufficiently to present a consistent or satisfying picture of village life. In fact, these chapters are appallingly tedious to read because of this lack of organized analysis. As in the rest of the book, Myers presents his data and generalizations in a jerky and disconnected fashion which makes it difficult to follow the argument. It is perhaps comprehensible, if not excusable, that many scholars seem to have snapped up his conclusions without searching for the evidence supporting them.

The central, pivotal conclusion, the one most cited by other authors and the one upon which most of his other conclusions depend, is the assertion that peasants in North China suffered no decline in living standards over the fifty years or so before the Japanese invasion of 1937. This is, he asserts, because Western economic penetration, so far from causing the immiseration of the peasant, provided through the possibility of urban wage labor and the cultivation and sale of cash crops an escape from the limitations of traditional technology and the pressures of population growth.

There is, however, *no direct evidence* for the assertion that living standards did not decline in the four chapters of village summaries upon which that assertion is supposedly based. So far as the reader is permitted to know, the Japanese researchers did not ask any questions which would permit even approximate evidence of the type needed; i.e. what, if any, change in the real or money income or in the consumption patterns of households had occurred in the memory of the villagers? The only acceptable (but inconclusive) evidence of this type in the book does not come from the Japanese survey at all, but from some of the statistics collected by John Lossing Buck in the 1930s!

There is, however, quite a bit of indirect evidence about living standards both in the analysis of the Japanese survey material in the front chapters and in the general survey of literature and research in the second third of the book. *Almost all of this indirect evidence attests to a definite decline in the living standards of many rural households.* The indirect evidence indicates a sharp increase in the incidence of the *t'an-k'uan* surtax after 1900, a decline in the land yield in one of the surveyed villages, testimony that terms of trade between city and countryside had worsened, the effects of civil war and social disorder, the introduction of crop-watching societies where in the previous centuries gleaners had been permitted to gather part of the harvest (what happened to the gleaners?), a shift to consumption of inferior food such as potatoes and millet instead of wheat, and the shift to growing of cash crops and the search for employment in the towns by farm families.

The last two factors, cash crops and off-farm employment, deserve special discussion because Myers uses them as the *deus ex machina* which explains why, according to him, living standards did not decline. On his own evidence, however, both expedients did involve hardship for the families which adopted them:

> *After partitioning of land, many new small farms might be too small to earn enough farm income to support the household, and this forced some household labor to be dispatched to work outside the village for an indefinite period of time. Although households considered this a great hardship, this alternative would be weighed and compared to the prospect of cultivating a cash crop, which involved hard work for the family, possible decline in soil fertility, and financial risk in the event of a decline in crop price [p. 201]*

It would seem in fact that both the adoption of cash crops and the sending of labor to the cities are not only indirect evidence of a decline in living standards, but themselves constitute part of that decline in that they involved a loss of leisure or an undesired separation of family members, or both.

Myers does not deny the existence of rural misery in China, although for the most part his language tends to obscure consciousness of it. To explain this misery, he cites three factors. First, population growth at an estimated average rate of one percent per year increased pressure on the available farmland. Second, land distribution was already very unequal at the beginning of this historical period, and while it remained very unequal throughout the period it became, if anything, slightly more equal in the course of the twentieth century as the very large landholdings were divided up. Third, "random disturbances" such as drought and warlord armies tended to knock the peasants down although they could hold their own economically in normal years.

Presumably no one would question the first point, and Myers supports the second with evidence which seems considerably more substantial than the assertions about living standards. It is plausible that, as a result of population pressure, holdings became smaller and more evenly divided in a society where every son had a claim to inherit some land. In such a situation it is also possible that the percentage of land rented might have declined, but this would probably not be because off-farm employment was more attractive than land rental (as Myers interprets it), but because as farms become smaller their owners no longer had any surplus which they could afford to rent out.

We must also examine for a moment the concept of "random disturbances" to the economy, which Myers uses to explain a good deal of rural misery and to which he devotes an entire chapter. Random disturbances, in his definition, "were not connected with the general pattern of internal development and cyclical change, and they occurred suddenly, without warning, to produce severe consequences for the rural economy. These disturbances were natural disasters, wars, increased taxation, and price fluctuations."

Some natural disasters are, of course, "acts of God." Others, particularly in a country dependent on carefully engineered irrigation works which may be misused or fall into disrepair, are at least partially man-made. Wars, increased taxation, and price fluctuations are definitely man-made, and they are more than coincidentally "connected with the general pattern of internal development and cyclical change."

The Japanese occupation of North China after 1937 was no "random disturbance." Japan was in China for the sake of the resources which could be obtained there for the use of the Japanese economy. Japanese imperialism was connected directly and intimately to the growth of the treaty port economy, foreign trade, and the spread of cash crops – in short to most of the phenomena which Myers is trying to portray as beneficial to the peasantry. The occupation of China was the means chosen to secure Chinese resources from the competition of other powers (Britain and the United States).

A major shortcoming of this book is its failure to examine with more than a passing mention the depression of the 1930s in the Chinese countryside. This depression had two phases: the first, lasting from roughly 1931 to 1933 was confined to the rural areas while the cities enjoyed a hectic, unhealthy boom; the second hit the urban economy with force as the very high prices offered by the U.S. Government for silver purchase sucked China's medium of exchange out of the country. It is hard to believe that the Japanese researchers collected no data on a historical event of such recent date, but Myers offers no analysis of these events which – however temporary or random he may consider them – could have provided valuable evidence on the economic relations, exploitative or otherwise, between town and countryside.

Finally, although Myers does not point out the connection, it must be noted that some of the farm boys who had to be sent out of the village to augment the family income with wage labor undoubtedly earned their living by plunder in the warlord armies which represented such a plague to the farm economy.

Myers devotes the final third of the book to discussions of the relationship of the peasants to landlords, merchants and moneylenders, and to the government. He attempts thereby to answer the question, "Was there a mechanism by which these wealthy classes exploited the peasantry, and if so, can the degree of exploitation be measured?" But his treatment of these questions must be rejected as wholly inadequate, for he has no operational definition of "exploitation" and can therefore offer no attempt to measure it.

* The ambiguity of the phrase "to rent land," which could have either a landlord or a tenant as subject, makes it difficult to discern Myers' argument precisely at many points when he makes statements about "peasants renting" land.

Landlords resident in the same village as their tenants are exonerated by him of the charge of exploitation because the rental of their surplus land tended to distribute evenly the available labor over the available land. In Myers' view, the peasant with excess land is doing his poorer neighbor a favor by letting him cultivate it.

> *The land tenure system must be considered as a system to equalize land use by informal agreement. It functioned to enable peasants with little land to farm more and those who could not farm their land efficiently to earn income by leasing it. [p. 48]*
>
> *The land tenure system in all four villages functioned to enable households with different land holdings to farm more efficiently and obtain more farm income. [p. 126]*

By Myers' own information, however, the tenants were allowed in some areas to keep only twenty or thirty percent of the crops they cultivated (pp. 93, 108). By refusing to distinguish between rich peasants (with land to rent out) and poor peasants (who have to pay high rentals to cultivate other people's land in order to survive), Myers by his use of language almost succeeds in obscuring this important distinction. He also suggests, against all common sense, that tenants who had to pay such steep rentals or send family members to the cities to find work were earning a surplus with which to buy land of their own, rather than battling for a puny subsistence.

Absentee landlords are similarly exonerated of the charge of exploitation. They obtained their landholdings from peasants who were not able to repay mortgages. Since mortgaging land was the only way to obtain credit in pre-revolutionary China, the rich landlord-moneylender-merchant class in the cities was thereby supplying an essential service to the peasantry and so, in Myers' reasoning, was justified in everything it chose to do. He asserts complacently,

> *Where land was transferred to another party as a by-product of the credit system, it did not lead to undesirable consequences for the rural economy, because land as a form of near money played an important role in the transfer of credit from lender to debtor. Without such a rural credit system, farm households would not have been able to farm throughout the year and reclaiming land would have been impossible. [p. 234]*

Myers' discussion of tenancy and landownership is fatally flawed by his failure to recognize that all transactions are two-sided; thus, if one peasant saves enough money to be able to buy some land, another must be in enough distress to be forced to sell. Except for areas such as Manchuria where virgin lands were being developed (and fortunes made — with which to buy land in other regions — see p. 231), the total stock of land must remain constant.

To generalize about "peasants" without further differentiation merely obscures the issue: did the introduction of cash crops, for example, benefit rich peasants more than poor peasants, landlords more than tenants, or vice versa? Myers' attempt to show that the percentage of tenant households was lower in areas which had ready access to off-farm employment and to markets for cash crops is a failure by his own admission (pp. 236-40); if anything, his maps show the opposite relationship, with higher rates of tenancy near railroads and port cities. But it is dubious in any case that the "percentage of tenant households" is a meaningful indication of rural poverty in the Chinese context, in view of the declining size of all farms and the drift to cities and to warlord armies of those who had lost all their lands.

While he leans heavily on J. L. Buck's statistics, he subscribes as well to what might be called the Pearl Buck thesis of the rise and decline of farm households.

> *Wealthy farm households accumulated land in three ways. First, a household through hard work and practicising rigid economy each year might be able to save enough to buy extra plots. If this same household were free of family sickness and there were several working hands in the family, it could increase the size of the farm. Second, a household head might save money by working in Manchuria or a distant city. With hard work and careful saving, this same individual might be able to start a small business in a city or market town. If this business prospered, the proceeds could be used to buy land in the householder's former village. Finally, a peasant household might be fortunate enough to obtain a windfall gain of cash from a relative or through some strange quirk of fate. [p. 231]*

The footnote to the last sentence of this paragraph actually refers to the incident of the discovery of silver in Pearl Buck's novel *The Good Earth* !

Two pages on, however, we are given the example of how a real-life windfall was used to build a personal empire of landholding, moneylending, and commerce:

> *Mou's early windfall gains were acquired from a speculative venture. During a great famine in 1917 the peasants of Ch'i-hsia county were reduced to eating bark off the trees. At the height of the famine Mou loaned grain to peasants in exchange for plots of land. In this exchange he gave only 230* shih *of grain for 3,500* mow. *In later years the annual rent collected from these lands yielded 650* shih *of grain. Each year Mou practiced arbitrage in the grain market to increase his earnings from the grain trade. During the harvest months when prices were at rock bottom, Mou purchased large amounts of grain and stored it in his granaries to sell during the spring months when prices rose. Mou had the reputation of cheating his tenants by having his rent collectors use grain measuring rods longer than those used in market towns. However, Mou devoted most of his efforts to managing his urban commercial and industrial holdings. . . . [pp. 233-34]*

Might this be regarded as exploitation? Myers does not pass judgment on Mou, but gives us yet another unsupported assertion that "In spite of the wealth and urban based business operations of these landlords, their domination of the rural community was exceedingly limited." He does not define "domination."

Nor, as he makes clear in the following chapter, is the speculative activity of merchants such as Mou to be criticized, for those powerful merchants who could afford to buy when prices were low and sell in times of scarcity were performing a social service by preventing the seasonal price fluctuations from being even more severe than they were. By inference, the starving peasants should have been grateful that Mou possessed

hoarded grain to lend, rather than complaining that he demanded all their land in exchange. Similarly, high profits for middlemen are justified by Myers because "these middlemen were vital to the trade because the absence of standardized market procedures and weights and measures made it necessary for merchant groups with specialized knowledge to operate in these primary markets and provide the connections with the secondary and terminal markets." (p. 253) Yet lower on the same page it is revealed that one of the "services" provided by these merchants was the adulteration of the cotton they sold! In the same vein, despite abundant evidence throughout the book of the parasitic nature of the Chinese bureaucracy and the steadily increasing tax burden, Myers complacently assures us that "because of the steady rise in farm prices, this did *not necessarily* mean a greater tax burden for the peasantry" (emphasis mine), and despite much evidence (from other sources) to the contrary, that "it can safely be assumed that village taxes were proportionately allocated to households according to their land or farm income."

In short, given the wealth of examples in his own book which he refuses to allow as evidence, it is not easy to imagine what Myers would recognize as exploitation. Those who were, and are, convinced that the laboring peasantry was exploited by merchants and moneylenders have never denied that the latter were performing functions that *in the context of that society and economy* were in a sense essential. It is precisely because such classes had a monopoly of such essential services as credit and marketing knowledge that they were able to make the profits which maintained them in wealth which the peasants could never aspire to.

There are, I believe, some valid points in Myers' study. There is no need to dispute his picture of the Chinese peasant (rich or poor) as a rational economic creature who was powerfully motivated to maximize income by the best use of all the resources of his household. He gives a valuable picture of the intricate multiple-cropping system of villages which lends credence to his contention that the peasants were careful and sophisticated cultivators. The smaller the little plots of land became, the heavier the burden of rents, taxes, and merchant profits, and the more intensively did the peasants farm their tiny bits of earth. The total productivity of the land need not have diminished even though the consumption of those who worked it directly did.

But nearly all of Myers' conclusions and inferences about the effects of foreign trade and urbanization on the peasant society must stand or fall on the truth of his central assertion that peasant living standards did not decline over the sixty-year period which his title claims to encompass. And anyone who reads the entire book carefully must be forced to acknowledge that the assertion is virtually unsupported. "There is no [or little] evidence . . ." is one of the insistently recurring phrases in Myers' work. Ironically, it can be applied most appropriately to the book itself.

The book by Hou Chi-ming is in a very different class stylistically and impresses me more favorably on second examination than it did when I first read it shortly after it was published. The first five chapters are literate and well organized presentations of useful information on the economic activities of foreign powers and businesses in China. Hou disaggregates "foreign investment" into foreign loans and direct investment, a very significant distinction, and provides tables showing the rise and decline of the various types of foreign capital participation over time. In my opinion he provides a more genuinely historical overview of his subject than Myers, for all the pathbreaking pretensions of the latter. For the most part, the information in the first half of the book is presented in a fair and ideologically neutral fashion.

To the casual or uninformed reader as well as the busy skimmer, however, it is the major themes and conclusions rather than the qualifying small print which make the lasting impression and which best indicate the intentions of the author. And my impression on first reading the book — that specious reasoning is here employed in the service of an emotional commitment — was not in error. The light shed in the early chapters gives way to the heat generated by Hou's attempt to prove that foreign contact could not have been harmful to the Chinese in an economic sense.

Even in the early chapters one can find a rather bad fit between his data and his conclusions. For example, he argues not only that the *per capita* amount of foreign investment in China was small, but that "even the absolute amount of foreign investment was not large." (p. 97) Yet the accompanying table shows that the absolute amount was the third largest for all underdeveloped territories in 1938, less only than the amount for Argentina and the combined total for India, Burma, and Ceylon. Similarly, he claims that foreign investment in Japan was almost three times as high as that in China in 1930 (p. 102), the implication being presumably that foreign capital is a good thing and China's problem was not getting enough of it. He does not, however, emphasize the significant difference that in Japan *direct investment* totalled less than 10 percent of the total amount, whereas in China direct investment (which was controlled by the capital suppliers rather than the Chinese) amounted to 78 percent of the total (p. 14). Furthermore, while the Japanese could control the use of the funds they borrowed, China's foreign borrowings were to a large degree for the purpose of paying indemnities imposed by victorious invaders, including the Japanese.

It is in Chapters Six and Seven that Hou shows the worst propensity for ignoring or explaining away evidence that is inconvenient for his thesis. This passage from pp. 143-44, which is accompanied by a table comparing costs in Chinese and Japanese cotton mills in China, speaks for itself:

> *Frequently cited evidence regarding the relative efficiency of foreign and Chinese manufacturing enterprises has to do with the cost of production of cotton yarns. As shown in Table 32, the reported data reveal that the unit cost of production of the Chinese mills was as high as 214 percent of that of their Japanese competitors. However, the evidence given is far from conclusive. First of all, it is not known how many firms were covered or whether they were representative. Second, the costs included are not complete and the two most important items are not included — cost of raw materials and depreciation. The cost of raw materials of both the Japanese and Chinese firms is not known, but the depreciation cost in the Japanese firms must have been substantially higher because of their high capital-worker ratio. (The higher labor cost of the Chinese firms apparently reflects the more intensive use of labor per unit of output.) Third, it is not correct to*

include interest charges in cost of production when comparing efficiency. According to modern accounting theory, interest charges are a reward to creditors of the firm just as profit is a reward to the owners. They are a device to allocate income rather than a cost of production.

Therefore, if allowances are made for the cost of raw materials (which usually figured as much as 70 or 80 percent of the total cost of production), the cost of depreciation, and the interest charges (which undoubtedly account for the largest part of the taxes and interest category in Table 32), one wonders whether there was any significant difference in total cost per unit between the Japanese and Chinese cotton mills.

The manner in which Hou argues away a very substantial figure with no supporting data illustrates one of the logical traps which abound in both his and Myers' books. A proposition is modestly and fairly introduced as "only a tentative hypothesis," but in the course of the argument the hypothesis is transmuted in the mind of the author, and the unwary reader, into a proven fact and is thus immortalized in the footnotes of other scholars. The controversial chapters in Hou's work are riddled with such unsupported "supposings." On page 169, in the key chapter on the fate of China's handicraft industries, Hou hypothesizes an increase in aggregate demand in China. Such an assumption is essential to his thesis that handicraft industries did not decline, for if imports increased, then local industry would necessarily have lost out if demand had declined or remained constant. But this assumption is no more convincing in the absence of supporting data than the opposite hypothesis.

Hou makes the valid point that the impact of foreign trade probably expanded the opportunities for some types of rural handicrafts while it usurped the market for others. But Hou's efforts to prove that there was no decline in the hand-spinning industry falls apart upon close exanination (pp. 173-77). His table 37 gives three different estimates of production and consumption of cotton in China, one for 1913 and the other two for different estimates made in the mid-1930s. In this table, the amount of cotton available for use in hand spinning is calculated by subtracting exports, consumption by factories, and direct traditional consumption (mostly wadding) from estimates of total production. The first point to be noted is that his method depends on the calculation of a figure which must by its nature be a residual category, and thus subject to all the cumulative, or mutually offsetting, errors made in computing the other categories. Such errors, judging from the wide variation in the figures given, must be considerable. He further assumes that all of this residual amount was in fact used for hand spinning; and since there are no figures for direct consumption for the 1913 estimates, he assumes that direct consumption remained constant, although all of the other categories fluctuated rather widely in that time period. Finally, of the two sets of data for the mid-1930s he accepts that which shows a higher total production and lower exports and factory consumption, thus giving a much higher residual figure available for hand spinning. He dismisses the other set of figures (which yields a much lower residual figure which would imply a decline in hand spinning) for a variety of reasons, all unquantified, all working in the same direction, and taken together not very convincing.

But the real surprise is yet to come. On the following pages we can, by careful reading, glean the information that the rapid increase in yarn imports (which presented the real direct threat to hand spinning) had actually taken place *before* 1913, which seems to make the preceding exercise irrelevant in any case!

One of Hou's main contentions in this book is that "foreign capital was largely responsible for the development of whatever economic modernization took place in China before 1937." (p. 130) There is no need to refute this statement, but merely to point out that it is meaningless because tautological. No definition is ever given of "modernization" which could analytically distinguish it from the effects of foreign capital and thus make the contention a testable hypothesis. Whether "modernization" was a good thing or not is a value judgment, which I would make dependent on findings about the effects it had on the livelihood of the common people of China. Despite the assertions of both Myers and Hou, they have in fact contributed very little to our knowledge of what happened in this respect.

Hou has taken great pains to show that Chinese enterprises, both "modern" (i.e. imitative of foreign factories) and traditional, were able to survive, coexist, and sometimes even show rapid growth in the shadow of the large foreign enterprises. His argument is convincing and I have no reason to question his conclusion in general, but when he infers from this that one cannot claim Chinese industry was "oppressed" by foreign capital, he has missed the point. The point is that *autonomous* development of the Chinese economy was prevented by the distortions of the treaty port economy.

The examples Hou gives of Chinese enterprises which survived or flourished in the early twentieth century comprise two basic categories: they are either enterprises which were auxiliary to the large Western firms, such as the handicraft industries which were stimulated by export demand (perhaps this represents the "archaicization" of foreign capital rather than "modernization" of China?), or types of factories which did not directly compete with their foreign counterparts. Several examples of the latter are given on pp. 152-54. Chinese cotton mills and tobacco firms produced coarse quality goods for mass consumption while the foreign firms dominated the luxury market; Chinese banks handled domestic business while foreign banks financed foreign trade and Western industry. One might draw the conclusion from this situation that Chinese firms had no chance at all to compete in the sectors controlled by Western firms and had to content themselves with less profitable lines of business.

Hou misses the point as well when he infers that because Western enterprises supplied mainly the Westernized treaty port economy and had little success in penetrating the rural mass market, therefore rural handicraft production did not suffer. Before the Western encroachment, rural folk had earned back some of the income taken from them in taxation, rents, and merchant profits by producing for the traditional "luxury" market. To the extent that the consumption of the wealthy classes switched to Western goods, the laboring classes lost this means of recovering (through more labor) the surplus which had been taken from them; instead, this surplus flowed overseas or into foreign banks in the treaty ports. And, if the terms of trade declined steadily throughout the period 1867-1936 (as Hou concludes on p. 197), then the wealthy

Chinese would have to extract ever greater amounts of surplus if they wished to maintain their standard of imports.

The works by Myers and Hou, while containing some useful observations and data, are particularly weak on precisely the controversial points for which they are most often cited as evidence. That these serious defects have thus far escaped the notice of previous reviewers and the scholars who cite their conclusions is rather alarming. Have we all been reduced to casual skimmers, accepting the conclusions of research without seriously examining their structural buttresses?

The subject of these works — the evaluation of imperialism and the effects of foreign trade — have such a pointed contemporary significance that ordinary critical faculties are easily blurred by emotion. Possibly in such a charged field we must all, myself included, frankly admit that rigorous objectivity is not possible. But those who are in agreement *a priori* with the conclusions are not likely to examine too closely the methodology, logic, and supporting evidence used to reach those conclusions; this task must be done by the opposition.

Though I have been deliberately harsh in my treatment of the two books under review, I have been uncomfortably aware that I am pitching stones from an edifice which, if it is not made of glass, does have some large and vulnerable windows. Myers and Hou caricature the anti-imperialist version of China's modern economic history in some respects, but they have in others only taken advantage of our own careless acceptance of hand-me-down truths. If the conclusions of these books are wrong, we must make the effort required to understand why and where they are wrong in order to assist our own further efforts.

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New Perspectives on the Subcontinent

by Gail Omvedt

Imperialism and Revolution in South Asia, edited by Kathleen Gough and Hari Sharma. New York: Monthly Review Press, 1973.

I.

Kathleen Gough and Hari Sharma's *Imperialism and Revolution in South Asia* marks something of a breakthrough in the field of South Asia scholarship: the first edited collection of Marxist writings by both Western and Asian scholars, journalists and activists covering most of the crucial issues in the field. It shows some omissions and contains some unresolved and not clearly delineated debates, but this is inevitable given the vast amount of work still to be done and the important questions of theory and practice still debated among Marxists; it has some unevenness in articles but at the same time contains some of the best of recent analysis. All in all it is a fine collection.

One might think it would be particularly welcome to South Asia scholars. The field is at present more or less in a state of theoretical disaster. The old paradigms of American-generated research – modernization theory, the stress on Western-educated elites, "sanskritization" as the major form of low-caste movements, caste pluralism as a foundation for democratic pluralism – all these have been found quite useless in developing a thorough analysis of ongoing developments in the subcontinent. Accordingly the emphasis in academic work has shifted from American functionalism to British empiricism: at present there is a rampant Namierism in the field generated, apparently, from England.[1]

Younger scholars are working on local studies to denigrate and deny the existence of large-scale movements, class solidarities and ideologies in favor of individualistic self-interest, patronage politics and factionalism. Older scholars who once analyzed nationalism in terms of emerging caste elites now feel constrained to apologize for even admitting the existence of a nationalist movement except in the diverse responses of a variety of groups to a single imperial bureaucratic framework. The result is some very interesting and detailed empirical work that is theoretically barren.

In this context of analytical uncertainty it might be hoped that scholars would be open to reconsideration of the themes raised by the Gough-Sharma collection, but apparently the resistance of the field is too great. Scholarly funds and institutes continue to be dominated by the old imperialists (the British) and the new (the Americans). The immediate

academic reaction to the book has been to scoff at the very idea of its central focus on "the dynamics of imperialism and revolutionary movements" (preface). It is difficult to deny the role of imperialism in shaping modern South Asia, but scholars do this by insisting that imperialism did not change Indian *society* but rather set up a united bureaucracy and political system to which the diverse and fragmented elements were forced to relate.[2] The stress on revolution is easier to deny, simply because most western scholars have taken from the beginning the position that revolution is impossible in South Asia, that the masses have had little separate interests or consciousness and no organization apart from caste/patronage /religious ties.

But where American and British scholars may be unwilling to deal theoretically with the disastrous effects of imperialism and its continuing neocolonial impact on South Asian societies, Asian journalists, intellectuals and even businessmen have recognized economic crisis when they see it.[3] And where academic elites may refuse to recognize the possibility of revolution, politically conscious Asian and Western intellectuals who have been aware of the massive 1968-69 student-worker strikes in Pakistan, the threat of revolutionary guerrilla warfare in Bangladesh, the 1971 Ceylon insurgency, the tremendous rise in rural class conflict in India since 1967, and the recent food riots and mass strikes may be ready to understand that there is some kind of mass force underlying these events and to give consideration to a theoretical analysis of these. Thus, while *Imperialism and Revolution in South Asia* is a scholarly book, it is likely to have its biggest impact, at first, outside the narrow academic field. And this is fitting, too, because the South Asia subcontinent remains one of crucial strategic importance, of great revolutionary potential in spite of its present misery, and one that has been in many ways more unknown even than China – for where China had kept observers at a distance for most of the period since 1949, India and other nations have welcomed them in but presented such a shifting illusion of appearances that few have penetrated to the mass realities underlying these.[4]

II.

What is the surface image, then, that South Asia presents at the moment to the rest of the world?

South Asia: four countries bound together in historical experience but torn apart by religious-linguistic conflicts and social-economic differences. India, image of the export of saffron-robed gurus to the West and of starved migrant laborers and merchant middlemen to Southeast Asia and Africa; image of the starving Calcutta orphan pictured in charity ads; a country of seemingly hopeless poverty and caste-bound indifference to life; a lackadaisical democracy and ineffective administration. Pakistan, image of the Muslim theocracy, of a militarism that finally erupted with fury in the east. Ceylon of Sri Lanka, image of Buddhist-Hindu disputes, image of the emerald isle suddenly disrupted with the bodies of slaughtered insurgents floating down the rivers. Bangladesh, image of ten million refugees fleeing holocaust, of disorganized resistance and more disorganized recovery, an international basket case. . . .

The international image of South Asia is one of weakness, religious mysticism and immiseration. For a time India was to be a showcase of Westernized democracy and development in contrast to China; for a time the "green revolution" was to solve the food problems of Pakistan and India alike. Now these hopes have gone; the *Far Eastern Economic Review* editorializes that "the international community is now treating India with the same contempt with which it views banana republics, nations which, manifestly, cannot cope with even the most fundamental problems of administration."[5] What is left seemingly is poverty, stagnation, sporadic but hopeless violence. . . .

The importance of *Imperialism and Revolution in South Asia* is that the contributors go beyond these surface manifestations of impoverishment and violence to dissect the workings of imperialism and class domination which have created the crisis. And they go behind the phenomena of class, religious and elite-led conflicts stressed by traditional social science to focus on the roots of class conflict in South Asia's changing rural society and to assess, very soberly, the strengths as well as the weaknesses and divisions of the revolutionary movements that alone can solve the crisis induced by imperialism.

In other words, Gough, Sharma and their contributors begin with two guiding hypotheses: revolution is *necessary* for South Asian development, and revolution is *possible*. The reason for the lack of welcome the book has met with so far in the traditional academic forums is that the traditional academic world has built itself on the opposing assumptions. The value of the Marxist analysis taken in this book must be shown in practice – not only in the revolutionary practice which will be the ultimate criterion of whether or not the society was "revolutionary," but at present in theoretical practice which justifies its beginning assumptions by the fruitfulness and relevance of analysis. Let the reader judge.

III.

The book contains articles both on diverse topics and representing diverse points of view. The next section will deal with some of the issues at debate; this section will summarize the major themes of the four parts of the book, which represent a sort of dialectical effort to deal with both the macro- and micro-level and both objective and subjective factors. The first part presents analyses of the political and economic structures resulting from imperialism and their development in the post-war period; the second gives a focus on the concrete daily life of the poor in three village studies; the third presents analyses on the "subjective factor," that is, the development of revolutionary movements in South Asia; and the fourth concludes with the interweaving of these forces in the major subcontinental crisis of recent years, that of Bangladesh.

The first part appears in many ways to be the most

thorough; only a discussion of the economic development of Ceylon is lacking to compare to those of India and Pakistan. Amiya Kumar Bagchi begins with a discussion of the effects of imperialism both in thwarting the growth of industrialism and preserving to a significant degree the position of absentee landlords and money-lenders in Indian agriculture, a structure which many describe as "semi-feudalism." After an analysis of the varying forms of relationship of India to British imperial needs, he notes that some changes occurred after the 1930s with tariff protection and some growth towards self-sufficiency in industry. But this, he argues, only inaugurated a period of "dependent capitalism" with a lessening role of direct British control replaced by more American and multinational influence and a growing foreign investment in manufacturing sectors. After independence he sees primarily an increase in dependency, resulting from elite unwillingness to put through basic domestic reforms coupled with a lack of inhibition against foreign collaboration, with balance of payments problems leading to a yielding to foreign pressures to relax controls on foreign investment. According to him, Soviet aid is insufficient to provide a way out, nor will Indian investment in other Third World countries help; only a thoroughgoing socialist revolution taking India "out of the capitalist orbit" will prevent further growth of multinational corporation dominance.

Paresh Chattopadhyay's survey of changes since independence corroborates and extends this analysis. In agriculture, a modest growth (in the last couple of years since the writing of the article this has come to a halt) contrasted with the complete stagnation of colonial years and resulted from minimal land reforms. Thus he sees a slow acceleration of capitalist production in agriculture, though without basic agrarian reforms, resulting in some disintegration of the peasantry, a growth of landlessness, and a large percentage of the agricultural area estimated at about two-thirds under commodity production. However, the disintegration of the old semi-feudal mode of production is hardly complete, he notes, with tenancy and sharecropping still highly dominant, with over 60 percent of the total farm credit coming from private moneylenders, and with a rise in the ratio of attached to casual farm labor. Thus gains made have not been sufficient either to establish capitalist production in agriculture nor to solve the problems of food production. In industry he sees some initial growth, again followed by alarming trends, towards the end of the second Five-Year Plan – with the growth in industrial production slowing down considerably, with the balance of payments crisis, and a rising net inflow of foreign capital as a proportion of total investment. The effects of the dependent capitalist path have been increasing inflation, declining rates of growth, and an increasing inequality of the distribution of whatever growth in income has occurred, with rising percentages of the population below the minimum standard of living. Like Bagchi, he stresses that these economic difficulties have resulted from the basic choice of the Indian bourgeoisie to accept foreign dependency and ally with semi-feudal elements in the countryside rather than to put through the basic agrarian reforms which alone could remove the obstacles to the development of the country's productive forces.

Hassan Gardezi analyzes briefly the development of Pakistani dependence in a similar fashion. According to him, Pakistan emerged as an economically solvent nation with substantial growth rates and a surplus in the balance of payments until about 1956. Then, with Ayub Khan's accession to power in 1958, came the rising dependence on foreign aid and technicians, with Pakistan becoming a "new type of economic colony." Thus the so-called "decade of development" in Pakistan has resulted in lesser growth and a decline in the real wages of urban and rural workers, as well as uneven development in which East Pakistan, now Bangladesh, was the main sufferer. Like the Indian writers, Gardezi stresses the internal roots of this increasing dependency in the maintenance of class rule by landlords, businessmen and the military-bureaucratic elite. Feroz Ahmed's article in this section provides a sensitive analysis of the changing class forces underlying recent political developments, particularly the role of Bhutto's Peoples' Party. Alavi's article in this section also focuses on Pakistan, but is devoted mainly to presenting his thesis on the "post-colonial state," an issue we shall discuss later.

Finally, in a very significant article Hari Sharma takes up the issue of the "green revolution" to look at the internal class forces underlying it. (He more or less accepts that the green revolution has resulted in some advance in production, an issue that is now very much under challenge.)[6] What Sharma argues is that the adoption of new technology by many richer peasants was *not* a result of governmental programs so much as of earlier land reforms and changes leading to a shift of class power in the countryside. These reforms were insufficient to benefit the masses of poor peasants and landless laborers, but they did cut off some of the power of the parasitic landlords and above all they consolidated the position of rich peasants both economically and politically. These new "kulaks," then, were ready to adopt whatever new technology they could find; the "green revolution" assured that this would come through governmental and Ford Foundation-financed programs. Again, Sharma stresses that "feudal elements" remain strong in agriculture in spite of these minimal reforms, but the changes have to a significant degree put the rich peasants on the side of the landlords and intensified the contradiction within the village between these and the rural poor.

In sum, the first section of the book establishes the theses that – contrary to the opinion of many contemporary scholars of South Asia – imperialism has significantly changed the shape of South Asian society and economy; that formal independence has not led to any breakthrough either in economic development or economic independence; and that both dependence and crisis continue to occur because of the unwillingness of the dominant elites – the bourgeoisie, landlords and rich peasants – to carry through basic reforms.

The section describing the "roots of struggle in the villages" presents three articles, one by Saghir Ahmad on Pakistan, and two by Kathleen Gough and Mythily Shivaraman which focus on the position of Untouchable landless laborers in the Thanjavur district of south India.

The section on revolutionary forces and movements is spotty. A major gap is the lack of discussion of the historical roots and development of both communist and nationalist movements; only Kathleen Gough's introductory article really takes up some of the issues as to why Indian communism took the form it did. Nevertheless there are some interesting themes. David Ludden's article on the nationalist poet Bharathi not only refers to the role of culture in revolutionary movements, but also indicates the degree to which radical nationalism was linked with themes of transformation of

traditional culture (for instance the position of women) — though the article touches only scantily on one of the most important questions, that of caste. Hamza Alavi's famous article, "Peasants and Revolution," reprinted here, does not discuss the role of party organizing so much as the important issue of the revolutionary contribution of various sections of the peasantry in a comparison of the Russian and Chinese revolutions with two major peasant uprisings in India. Alavi argues that the middle peasant, not the poor peasant, is initially the most militant; poor peasants represent the strongest revolutionary force which must be mobilized, but they cannot at first be mobilized on their own because they are so much under the dominance of the village elites. He sees this mobilization as occurring through first rousing the more independent middle peasants against the landlords, and through the intervention of a red army which militarily destroys the power structure.

Most significant for South Asian revolutionary strategy is his argument that poor peasants can never be mobilized through parliamentary strategies which leave the village power structure intact. But this leaves two major issues: How are middle peasants to be initially mobilized in the post-independence situation depicted by Sharma and others, with the rural poor confronting a class alliance of rich-peasant kulaks and landlords? And, in the absence of a red army, will it be militant extraparliamentary class organizing or guerrilla terrorism organized by small bands that can most successfully shake up the village power structure and initiate a process leading to true armed struggle?

These issues are taken up in the two following articles. Mohan Ram, who surveys the development and splits within Indian communism since independence, and a Naxalite writing under the pseudonym "Inquilab Zindabad" introduce readers to the major debate going on at present among pro-Chinese communists in South Asia: whether the revolutionary vanguard should stress first mass organizing as a basis for armed struggle, or whether guerrilla struggle must precede and make possible the creation of mass organizations. The latter thesis is the notorious "annihilation of class enemies" line of Charu Mazumdar, and it is currently under heavy criticism by most of the remaining CPI(M-L) leaders now under trial in conspiracy charges at Parvathipuram.[7]

In the case of Bangladesh — the culminating section of the book and of the themes of developing revolutionary crisis — there is little theoretical disagreement in the analyses presented. Almost all writers agree that a colonial type of exploitation existed; that the outburst when it came was under the control of petty-bourgeois elements, with the Awami League not only installed in power with the help of the Indian army but also with the aid of pro-Soviet forces which accepted its hegemony; and that Maoist and independent guerrillas could do little to affect the outcome of the struggle. But they also agree that the crisis will, if anything, intensify with the formation of an "independent" state, that Bangladesh remains economically weak and dependent on India. Gough points out that the crisis has raised the national aspirations of other South Asian peoples, including Kashmiris, Nagas and Mizos and the West Bengalis themselves, in other words that "national liberation" is still on the agenda. Alavi concludes his article on the state by stressing the weakness of the Bangladesh state structure and the fact that large quantities of arms are still in the hands of popular forces: "it is equally possible that Bangladesh may be plunged into an armed revolutionary struggle." (p. 172) And it can be noted that recent journalistic accounts indicate a rise in armed insurgency in sections of Bangladesh, with four "Maoist" guerrilla forces working in alliance, with the Awami League and pro-Moscow groups allied against them, and with Indian officials deeply worried that the Bangladesh military by itself cannot defeat the insurgents: the stage is perhaps set for further imperialist intervention.[8]

IV.

But if there is little disagreement over the analysis of Bangladesh, there remain crucial areas of theoretical debate within the articles collected in *Imperialism and Revolution in South Asia* The editors note that they include "a variety of viewpoints," but this is not simply Marxist pluralism: in fact, the disagreements really fall into place on either side of a single overriding issue. And that is the question of the nature of the South Asian states, the role of the "national bourgeoisie," and the issue of a "one-stage" versus a "two-stage" revolutionary process.

The "one-stage" theory would hold that South Asian bourgeoisies are totally in league with imperialist forces and no alliance with them is possible in the "post-colonial" period; that "capitalism" rather than "semi-feudalism" characterizes most of the agricultural structure; and that the revolutionary alliance must consist of workers, poor peasants and revolutionary intellectuals fighting immediately for socialism and by-passing the "democratic" stage which these theorists argue leads into "tailing" the bourgeoisie.

The advocates of a classical Marxist form of a "two-stage" revolutionary theory[9] hold in contrast that an alliance with some sections of the bourgeoisie is possible but only if revolutionary forces, based on workers and poor peasants, have hegemony; that because agriculture is not totally capitalist but remains "semi-feudal" and because independent economic development has still not taken place, the first stage can focus on the demands for an end to imperialist rule and for "land to the tiller" and the release of productive forces brought about by a true agrarian revolution.

The problem is that these issues underlie many of the articles in the book, but the debate is never totally clarified. Most of the contributors appear to accept some form of a two-stage revolutionary theory, but the major theoretical articles by Gough and Alavi explicitly argue against this by stressing that the bourgeoisies of India and Pakistan have lost their oppositional role and have become thoroughly allied with imperialism and "feudal" large landowners (or "kulak" rich peasants). Gough in addition argues that agricultural relations are characterized by something like capitalist relations of production, and thus the notion of abolishing "feudalism" in agriculture through revolutionary land reform in the first stage does not apply. (On this issue Sharma and Chattopadhyay are ambiguous: they show the development of capitalist trends and the consolidation of a "kulak" class, but they also stress the prevalence of feudal elements — and they do not take up the larger issue of the relevance of this to revolutionary

strategy).

In their articles, both Gough and Alavi partly, and inadvertently, distort the two-stage theory. Gough does so in saying that it involves the acceptance of a "dual economy" thesis and a "mechanical" separation of the stages of national independence and socialism (p. 16). In fact, the two-stage theory does not do this, and it does not imply a "nonsocialist" third way of development. Rather, it stresses completing certain tasks (national liberation, the abolition of landlordism) as part of the first stage and in alliance with nonproletarian forces, but that this must be with proletarian hegemony and lead in an "uninterrupted process" into the socialist stage. Alavi distorts the theory by saying that "classical Marxist" theory sees the bourgeoisie as *necessarily* anti-imperialist; thus he says it needs revision because in "post-colonial" societies the bourgeoisie has fundamentally changed and become collaborative (pp. 162-65). This again is wrong; the classical theory saw the bourgeoisie as "oppositional," "Janus-faced," vacillating – not purely anti-imperialist; an accession to some power will maximize its collaborative characteristics and weaken its oppositional ones, but will not change its fundamental nature as long as dependence and neocolonialism remain. The crucial issue seems to be whether colonial dependence has in fact ended – and almost in spite of his own analysis of the continuing power of the "metropolitan bourgeoisie," Alavi belives that the first stage of national independence has been accomplished (otherwise why call it a "*post*colonial" state?). Thus he says the "winning of national independence" and the "institutional and legal framework necessary for capitalist development" have been completed (pp. 162-63). If that is true, the bourgeoisie would indeed have no more reason for opposition.

Let us look more closely at this issue. Alavi, Gough and others who argue a "one stage" theory are led to their position because the "national bourgeoisie" is apparently in power in South Asian societies and is apparently quite in league with imperialist forces against the interests of the masses in their own society; the immediate main contradiction in these societies appears to be internal class conflict, and poor peasants and workers face the national bourgeois state and rich-peasants/landlords as their immediate enemies.

This in a sense is true, but the issue is not so simple. After all, in the greatest recent conflagration on the subcontinent, the case of Bangladesh, the immediate contradiction was not internal class conflict. The peasants and workers of Bangladesh were not, at that moment of revolutionary crisis and developing armed struggle, fighting directly against their own bourgeoisie. Rather they were facing the Pakistani army (as well as landlord gangs) and their immediate task (in which the revolutionary forces could not succeed) was to prevent petty-bourgeois and bourgeois forces from controlling and cutting off this struggle. If there is a "national liberation" element involved, then the direct and immediate fight is against the external enemy, and the problem of the proletarian forces is to win control of the total revolutionary process and to ensure that the struggle is not cut off halfway. The two-stage theory stresses that at this point petty-bourgeois and some patriotic bourgeois elements will support the liberation struggle because of their desire for national independence and economic development, and their support is maximized in a united front which takes these goals as its immediate program.

In fact, the situation of South Asian societies since the Second World War can perhaps be understood in analogy with Mao's characterization of the multiple and shifting contradictions in Chinese society in his essay *On Contradiction.* Here he points out that in the general period of the new democratic stage there were many contradictions and conflicts, the principle ones being those between the Chinese masses and feudalism, and between the people and imperialism. In the civil war period before the Japanese invasion the internal conflict became the primary one; but once the invasion occurred an anti-Japanese united front became possible and the Communist Party could win over many patriotic bourgeois-petit bourgeois elements and base its victory on both its ability to carry through land reforms and its decisive leadership of the national struggle. In other words, the focus of the princple contradiction shifts.

We could analyze changes in South Asia similarly. In the period immediately following formal independence the colonial power retreats, to an extent, dominance takes more subtle forms, and the national bourgeoisie comes into power with a program of "independent development" and social reforms, especially land reforms. In the period of apparent colonialist retreat the main contradictions now seem to be internal, particularly as the native bourgeoisie and rich peasant groups consolidate their power.

But – and this is the crucial factor – this does *not* lead to economic independent development, it does *not* mean the end of imperialist controls. Because the bourgeois forces which controlled the independence movements are unwilling to carry through basic reforms, economic neocolonialism reasserts itself, feudal elements remain in agriculture, economic crises transform themselves into all-round social crises, and the comprador nature of the bourgeoisie grows stronger with collaboration agreements, the relaxation of controls against foreign investment, etc. (The contributors to the Gough-Sharma book have documented this process.) As a result, revolutionary movements grow in strength, both in terms of opposition to continuing imperialist dominance (or the dominance of sub-imperialist groups over oppressed nationalities) and in terms of the resistance of workers and poor peasants to the power of native elites. The local ruling classes find it increasingly difficult to contain these movements under democratic forms of government. As a result, military-bureaucratic power grows, political dictatorship may replace democracy, and direct foreign intervention becomes necessary. Therefore both in economic terms, with increasing crisis and stagnation, and in political terms, with the democratic gains of the independence movement being wiped away, the situation appears more and more a return to that of direct colonial rule. Thus the nature of the principle contradiction shifts, and the anti-colonial "nationalist" factor comes into play. This, then, becomes the basis for the anti-imperialist united front of the two-stage theory.

Even though she argues for a "one-stage" approach, Kathleen Gough recognizes this process in her important introductory article on South Asian "revolutionary potential." The paragraph is worth quoting:

> *This does not mean that the revolutionary forces must necessarily "fight the whole lot at once." Although they have proved able to organize localized class struggles by tribal people, low-caste people, poor peasants, landless laborers, and urban poor against their immediate*

oppressors, the South Asian revolutionary leadership may not be able to mobilize the people en masse over large areas until the military has assumed political power in all three countries and until alien intervention has provided external enemies. Such "external enemies" may come initially from linguistic groups located around the metropolises within each nation and be deployed against outlying groups of different ethnic origin in the more exploited hinterlands, as happened with West Pakistan's invasion of East Bengal. The rapidity with which the major industrial powers rushed military aid to Ceylon in the recent crisis suggests, however, that "external enemies" will eventually be supplied from outside each nation by one or another of the superpowers and their satellites . . . After such invasions, patriotic people from all sections of the indigenous propertied classes are likely to cross over to the side of the poor peasants, the landless laborers, the revolutionary youth, and the working class, as happened in China and Indochina. (p. 17)

The point about this process is that sections of the bourgeoisie do not "cross over" automatically; rather, their support for revolutionary workers and peasants is won in a united front-national liberation program — *as happened in China and is happening in Indochina.* It is precisely the "two-stage" approach, not the "one-stage" one, that recognizes and prepares for this eventuality.

V.

At this point it might be useful to mention what appear to this reviewer to be some of the most important gaps or omissions in the analyses presented in *Imperialism and Revolution in South Asia.* These are not mentioned as a critique of the book — no such collection can cover everything, and Gough and Sharma do a splendid job of covering a great deal — but rather to indicate what may be future important lines of research. Indeed, there are issues on which the Marxist movements of South Asia have perhaps not been fully clear.

One is the general issue of linguistic nationalism — within India and Pakistan as well as between the different states of South Asia. Communists have long accepted the idea that the subcontinent is composed of numerous nationalities, each with its own language, a well-developed literature, and varying cultural patterns. Indeed, communism within India itself shows tremendous variations, and has historically, between the different linguistic states. At the same time, demands for separate states within the Indian union have been an important part of India's early post-independence history, particularly in the case of Andhra and Maharashtra. Marxist theory also stresses the right of a dominated nationality to move ahead independently of a reactionary central government whose policies hamper the development of democratic and socialist forces. Bangladesh in particular brought this issue to the fore, not only with the question of the breakup of Pakistan, but also in the threat felt by many in India of the possibilities of coordination with revolutionaries in West Bengal: the spectre of a "united red Bengal" troubled many conservative analysts.[19] Yet no clear theoretical or strategic line on this issue has solidified among South Asian revolutionaries as a whole.

What is needed perhaps is a full concrete historical analysis of the development of the different nationalities within the subcontinent, their varying cultural patterns and the factors underlying these, and the effects of the uneven development fostered by British imperialism. This could provide a background for an analysis of the role of this factor in the momentum of revolution on the subcontinent.

Another crucial issue is that of caste and culture. Now, a good deal of nonsense has been put forward about caste by bourgeois social scientists, and Gough and Sharma are right to stress, in opposition to this, the more fundamental features of class. Yet caste remains important in Indian society, perhaps to the same degree as (and with some analogies to) race in American society. The difference is that where the international communist movement could prod white American communists and white South African communists into recognizing the importance of Blacks and of the racial issue in terms of a colonial model, this did not happen in the case of India. Communists outside of India, for quite natural reasons, did not really understand caste, and the British — who played the most important tutelary role regarding Indian communists in the early decades — were perhaps not too sensitive to this issue. The result was that few of the early Indian communists were forced to confront the fact that they were not merely petty-bourgeois in origin, but high-caste as well (generally Brahmans), and this meant crucial inadequacies in their ability to understand and relate to important low-caste-based social movements in their own society.

Significant anti-caste movements occurred in India throughout the 1920s and 1930s; and these can easily be analyzed in Marxist terms as "anti-feudal" movements directed against both traditional cultural bonds as well as against landlord-moneylenders in the countryside. Yet many early communists found it much easier to understand the role of "bourgeois nationalism" than this low-caste "anti-feudalism." Thus, while in some areas communists have succeeded in upholding the cultural as well as class interests of Untouchable landless laborers — the most oppressed social group — in most cases this has not been true, and at crucial points the Marxist leadership has alienated important nonBrahman and Untouchable leaders such as Dr. B. R. Ambedkar. Yet, for obvious reasons, movements of Untouchables and low-caste groups continue and often take quite militant forms, as in the case of the recently organized Dalit Panther movement in Maharashtra ("dalit" means "oppressed" but is a synonym for the scheduled castes). Further, it is these low-caste movements — from the Tamil Self-Respect movement and the Maharashtrian Satyashodhak movement to the Untouchables' Neo-Buddhism and Dalit Panthers — which have raised most strongly the issues of *cultural revolution,* the role of traditional culture in maintaining bourgeois/landlord/rich peasant hegemony, and the role of a modern anti-caste culture in revolutionary movements. As Dr. Ambedkar warned Indian socialists in 1937 (at a time when Communists and Socialists were working together in the Congress Socialist Party):

That the social order prevalent in India is a matter which a Socialist must deal with, that unless he does so he cannot achieve his revolution, and that if he does achieve it as a result of good fortune he will have to grapple with it if he

wishes to realize his ideal, is a proposition which in my opinion is incontrovertible. He will be compelled to take account of caste after revolution if he does not take account of it before revolution . . . turn in any direction you like, caste is the monster that crosses your path. You cannot have political reform, you cannot have economic reform, unless you kill this monster.[11]

Tribals, too (*adivasis, girijans*), represent a crucial oppressed section within South Asia. They are among the most exploited of agricultural laborers, losing their land to caste Hindu landlords; they have been for decades a source of anti-colonial movements, separatist movements (as in the case of the Nagas and Mizos) and communist-led class revolt. In fact, the areas in which the CPI (M-L) obtained any kind of territorial base of armed struggle were all tribal areas;[12] indeed, Naxalite theorists do not appear to have quite come to grips with the fact that they have been unsuccessful in carrying forms of armed insurrection in India into almost equally impoverished caste Hindu areas (only in Bangladesh, where the national factor interceded, have they moved out of tribal areas). Yet, while tribal peoples are obviously crucial in a revolutionary process, it has to be remembered that the situation is quite different from that of China, where minority nationalities occupied 60 percent of the area even if they represented only 6 percent of the population. In India, tribals make up about 7 percent of the population, but occupy relatively small areas, areas that are quite accessible and vulnerable to transportation, communication and government control. Here, too, more analytical and empirical work is necessary to establish the strategic and social importance of various special groups.

The issues of the super-exploitation of oppressed groups such as Untouchables and tribals, the issues symbolized by the stories of Shambuk and Ekalavya in the Hindu epics, issues rising around the problem of developing true revolutionary leadership from the lowest sections of society and not simply upper-caste intellectuals – these issues have underlain much of the dynamic of developing revolutionary movements in South Asia, especially India, and these issues remain. Nevertheless, the ferment revealed in the movements of such groups is also making its contribution to the process of revolutionary unity.

Finally, one more omission might be mentioned: while the Gough-Sharma book goes deep into analyses of peasant society and transformations among the peasantry, there is still very little on the development and position of the Indian working class, its particular reactionary role, obstacles confronting it (e.g. bureaucratized unions controlled by reformist leadership) and the means by which these obstacles may be overcome. Historical or sociological studies of Indian workers, generally, are either lacking or shallow – something that might be expected from academic scholarship but which it is hoped that Marxist analysis might remedy.

Kathleen Gough and Hari Sharma describe their book as a "modest attempt" to meet the lack of studies of the dynamics of imperialism and revolutionary movements. In this they have certainly succeeded. In spite of occasional omissions, and with all the unresolved issues of debate and occasional unclarity in the articles collected here, *Imperialism and Revolution in South Asia* remains an indispensable beginning point, not only for students and general readers but also for research scholars. The Monthly Review edition is, as always, nicely put together and now that the paperback edition has come out the price will be within the reach of students for class use.

Notes

1. See especially the recent special issue of *Modern Asian Studies* 7:3 (1973).

2. *Ibid.*, Anil Seal, "Imperialism and Nationalism in India," pp. 321-47.

3. See for instance the various articles in the *Far Eastern Economic Review*, especially the special section, "India: Man-Made Crisis" (June 11, 1973); the survey of agricultural non-development in the 1972 Annual Number of the Indian business journal *Commerce*; and the various articles in the Bombay publication, *Economic and Political Weekly*.

4. Concretely, the very social organization of western scholarship in countries like India, Pakistan and Ceylon (sometimes referred to as "academic colonialism") has meant that researchers have been tied in with local elites, exposed socially only to the views and ideologies of the upper classes, and generally travelling in villages in the company of local elites and government functionaries. Another fact is that most of the better-known village studies have been done in the relatively quiescent period of 1955-65, prior to the recent upsurges. These academic pictures of village India might be compared to a book such as *Interview with India* by Frederick Mueller, a journalist who actually traveled through the rural areas on his own immediately after independence, and uncovered despair at a sham independence controlled by local merchants and landowners, class conflicts, and revolutionary upsurges. This book appears unbelievable to anyone brought up on traditional scholarship and the western image of India. It was unbelievable to me at first – until I had the experience of traveling in village areas in somewhat the fashion that Mueller did.

5. *FEER*, June 11, 1973, p. 7.

6. There has been some debate among social scientists as to whether the benefits of the "green revolution" have extended to the masses; most would probably now accept the general assertion – first made most strongly by Francine Frankel, *India's Green Revolution* (Princeton University Press) – that it has not and has resulted in intensified class conflict in the countryside. But now even the increase in production is in doubt and the possibility of food self-sufficiency is questioned. *Commerce*, for example, points out for India that effects have been minimal, that per-capita agricultural production has grown at a lesser rate since the introduction of the improved technology, and concludes, "there is absolutely no evidence for the green revolution." (1972 Annual Number, p. 9) And this, it may be noted, dealt with growth *before* the recent famines and food crises.

7. The Parvathirpuram Conspiracy Case trial in Andhra Pradesh brings together 68 leaders and cadres of the CPI(M-L), including six original central committee members. With the government producing record numbers of witnesses and documents, it is the major conspiracy trial in India since the famous cases instituted by the British government against Indian communists and workers in the 1920s.

8. See "Bangladesh: War against Maoists," *Economic and Political Weekly* (November 17, 1973), and *The Guardian*, December 26, 1973, p. 20.

9. This might be called a "Maoist" form in terms of the inclinations of most of the contributors to the book; none as far as I know would accept the Soviet version of two-stage revolution which stresses peaceful transition, the use of parliamentary forms, and the possibility of accomplishing the first stage under the hegemony of petty-bourgeois parties.

10. See for instance Justus van der Kroef, "Indian Maoism, Peking and Bangla Desh," *Studies in Comparative Communism* (Summer-Autumn 1972): "Bengali nationalism and the issue of a separate Bengali state are and remain the hidden factor in the continuing confrontation between the Naxalites and their overt enemies the CPM, on the one hand, and the Gandhi government and its CPI allies, on the other, just as it is a principle background dynamic within Bangla Desh as the Rahman Government seeks to checkmate its Maoist opponents." (161)

11. B. R. Ambedkar, *Annihilation of Caste* (Panjab: Bheem Patrikar Publications, 1968), 36-7.

12. See Biplab Dasgupta, "Naxalite Armed Struggles and the Annihilation Campaign in Rural Areas," *Economic and Political Weekly* (Annual Number, February 1973).

Short Reviews

The East Is Red, A Simulation of a Sino-Soviet War, designed by Redmond Simonsen and James Dunnigan. Published as an insert to *Strategy and Tactics,* No. 42. New York: Simulations Publications, 1974. $7.00.

It may seem a little odd to write a book review of something that is not a book. It may seem a little odder to review a "war game" for a publication that has such deep roots in the peace movement. But the study of modern Asia is, in large part, the study of conflict, and games are a valid tool for the analysis and discussion of conflict. Although it was intended for a rather narrow circle of war-game hobbyists, *The East Is Red* is a game that has much to teach the student of contemporary China.

For those unfamiliar with the esoterica of simulation gaming, a bit of explanation may be in order. The basic assumptions of military (or political) gaming are that the relative offensive and defensive strengths of opposing forces can be quantified; that these strengths can be compared, yielding simple, whole-number ratios ("odds"); and finally that from these "odds" we can generate statistical tables of likely outcomes for each "battle." A randomizing element (usually dice) is used to "resolve combat." As in any statistical exercise, you get out very much the same as you put in. When a great deal of sophisticaed research, analysis and development are "put in" to a game, a surprising degree of realism "comes out." A recent effort by Simulations Publications titled *Sinai* proved to be a terrifyingly prophetic simulation of the recent Mideast war in its course and outcome.

The forces deployed on the game board represent current Western intelligence estimates of the available Soviet and Chinese forces. An absolute limit is imposed on the Soviet "player" by the supply capacities of the Trans-Siberian Railroad and the port of Vladivostok. Various rules simulate the effects of nuclear weapons (rather chilling little markers bearing mushroom clouds represent "nuclear contaminated areas"). In most "scenarios" the game ends with the battered Soviet mechanized corps clinging desperately to the ruins of Shenyang and trying to beat off repeated Chinese mass counterattacks.

"Victory conditions" are somewhat problematical in this situation. The game designers settled for a point system based on the number of industrial complexes overrun or cut off by the Soviets before attrition and mutual exhaustion put an end to the ten-week campaign. An optional rule allows North Korea to join the Chinese, but the intervention of Mongolian, Indian or Kuomintang forces is ignored for the sake of simplicity.

A thoughtful, carefully researched article on the history and present state of the PLA and the Soviet Army accompanies the game.

Western radical intellectuals have chronically been caught unprepared and uninformed by events like the Czech crisis and the repeated Mideast wars. A natural abhorrence of things military often leads us to deprecate the role of violent force in world affairs. If and when the fighting breaks out again along the Amur and Ussuri, it is quite probable that most of us (purporting to be "China experts") will have no analysis, little knowledge and even less understanding of what is involved. Playing games may not be the answer; gaming, as we all know, has been widely abused as a tool of Cold War social science. But readers of *The Protracted Game* (Oxford, 1969), Scott Boorman's provocative "wei-ch'i interpretation of Maoist revolutionary strategy," may recall that Mao Tse-tung's own military thought is greatly influenced by paradigms and metaphors drawn from a traditional Chinese war game.

Michael Markowitz

Farewell to Manzanar, by Jeanne Wakatsuki Houston and James D. Houston. San Francisco: Houghton Mifflin, San Francisco Book Co., 1973. 176 pp.

Nisei Daughter, by Monica Sone. Boston: Little, Brown, 1953. 238 pp.

Any understanding of how the World War II relocation of 110,000 Japanese-Americans affected these people and their descendants has depended for too long on writers such as Daniel Iwao Okimoto and Bill Hosokawa. For these writers, the Japanese-American is at best a "miniature giant" or a creature "in disguise." His sense of self is submerged within a group, and his emotions are totally sublimated. He is a cultural schizophrenic, willing to outwhite the whites to make it in American society while desperately attempting to retain the culture of Japan to apologize for his differences. "Camp," which greatly affected the Japanese-American sensibility, is a vague memory – a mass exodus of people to desolate inland areas, justified by a sequence of social, economic, military and political developments. Of the thousands of people interned,

only the "Go for Broke" 442nd combat team and prewar and postwar individual successes are "good enough" to be remembered.

Now, however, Jeanne Wakatsuki Houston and James D. Houston demand recognition of politely forgotten and intentionally overlooked perspectives of camp life. In *Farewell to Manzanar,* they go beyond Hosokawa's unassuming camp structures. The accused and the humiliated internees voice the human experience of relocation and its aftermath. The twelve Wakatsukis, their friends, and their acquaintances are strong enough and great enough to express an entire repertoire of emotions. Mama smashes the beautiful dinner set at the white man's feet in silent retaliation. Seven-year-old Jeanne laughs and cries when only a child unashamedly can. Each person defines his or her own sense of worth; their battles are individual and unique. They never lapse into being "slant-eyed Oriental faces," a desensitizing catch-all phrase which facilitated the incarceration of an entire ethnic group.

Jeanne Wakatsuki Houston dares to discuss, for example, the emasculation of the Japanese-American style of manhood. Papa was a jack-of-all-trades, and he did everything with a flourish and with grandeur. Everything he did demanded an audience. Camp for Papa was blatant confirmation that he had no rights, no power, and no command over his own life. The accusations of disloyalty and the humiliations hurled at all Japanese-Americans devoured his personal style, leaving him powerless. The closing of Manzanar symbolized his spiritual death for he was stripped of his right to dream and hence of his will to make dreams a reality.

Understanding her husband's imposed castration, determined Mama strived to sustain family unity, despite the imposing mess hall meal shifts and cramped barrack quarters which forced one out of, rather than invited one into, the sixteen-by-twenty-foot unit identified as home. Her day-to-day experiences with communal washrooms, knotholes, and living quarters illustrate the stabs at human dignity which further degraded the Japanese-American sensibility.

For seven-year-old Jeanne, Manzanar was the major force molding her developing Japanese-American sensibility. Until the bombing of Pearl Harbor, she had never associated being Japanese-American with being "different." Most of her camp memories are of people she met and things she tried to do, which were all efforts to find hope, security, and something to believe in. Camp subtly and sharply conditioned her to all that it meant to be Japanese-American: internalizing the shame, humiliation, and guilt of internment, longing to be on the acceptable side of the fence, and hoping for a chance to again prove oneself to a nation by which one had been wrongly accused and punished.

In the immediate postwar years, Jeanne Wakatsuki Houston's experiences made conscious all that she'd internalized at Manzanar. Like Daniel Okimoto and Monica Sone, she quickly learned that people first acknowledged her as an "Oriental face," a mask which prevented them from seeing a human being. As a young woman, she could easily arouse the fascination and curiosity of white men, the significance of which she could fully understand only many years later. She could at best be second rate, allowed to excel only in restricted areas. The resulting conflicts of growing up Japanese-American simultaneously made her hate and reject the Japanese element in her and intensified her desires for acceptance by the larger society.

Sone

Twenty years ago, Monica Sone wrote in *Nisei Daughter* that being Japanese-American had its imposing limitations. The obi restrained one from doing cartwheels and Japanese social propriety interfered with a rough-and-tumble skid row lifestyle. Monica Sone rejected the Japanese language and the culture that her parents imposed upon her, as they could be useful only in certain limited situations. All too often they restricted one from healthy childhood desires: ballet dancing, helping oneself to food unceremoniously, and play-time after school. Rather than confirm Mama's and Papa's steadfast belief in the importance of Japanese culture, the family's trip to Japan only accentuated its uselessness to Japanese growing up in America. Their clothing made the Itois conspicuous among the Japanese as did the children's alley-style behavior. Rebuffs from Japanese children, expected and unnatural roles of behavior, and little appreciation for respected shrines and temples intensified Monica Sone's feelings of alienation among people who looked like her.

Whereas the relocation camp experience first initiated Jeanne Wakatsuki Houston to the uniqueness of being Japanese-American, it was the culmination of a restricted lifestyle for Monica Sone. She earnestly wrote of the frustrations of young, ambitious Japanese-American males during that time. American colleges granted them degrees while the greater society withheld its better jobs. "You can't waste a man's talents and brains without wrecking his spirit." (p. 122) Monica Sone herself was told to restrict her job choices to the Japanese community. The society which imposed restrictions and second-rate classifications then insisted upon reclaiming the emotional and material wealth of an entire ethnic group.

Now, seemingly as an echo to Monica Sone, Jeanne Wakatsuki Houston resolves her identity problem by rejecting dual personality. As with Monica Sone, Japan had little to offer her in any compensatory sense. Jeanne Wakatsuki Houston didn't speak Japanese, she smiled too much to be an odori dancer, and she considered the aristocratic Japanese woman with the rice-powdered face as diseased. Her desire to be a Girl Scout, a majorette, and a carnival queen were personal "all-American" aims and dreams, for which she alternately experienced gratitude, apologies, and failure, intermingled with her successes. Only many years later was she able to realize the price of human dignity she was willing to pay for acceptance, as she had internalized the greater society's ideals and standards as morally absolute. Like Monica Sone, she learned to reject the white racist love game she had initially played. She strived to understand herself as an individual with two cultural forces simultaneously acting upon her.

Indeed, in *Farewell to Manzanar* the Houstons succeed in articulating a Nisei soul, complete with its emasculated style of manhood and exotic and alluring female sexuality. Through personal incidents and human experiences, they provide an understanding of the costs of a people's struggle for acceptance as a means of survival, and they describe the imposing and subtle nature of white racist love. Like Monica Sone, they write of the relocation experience in human terms. As one reads, one also laughs, cries, worries, smiles, and hopes.

They remind us, as did Monica Sone twenty years ago, that behind expected "inscrutable yellow faces" are people with hopes, emotions, and will.

Joanne Harumi Sechi

The Pentagon Papers: Critical Essays, edited by Noam Chomsky and Howard Zinn, with an Index to Volumes I-IV. Volume V. Boston: Beacon Press, 1972. 413 pages.

It has always disturbed me to see a new bibliography filled with books I'll probably never read and challenging me to "use" them. Thus, rather than tossing out a 12-page list, I want to take a topic or group of books and explore the readings and the possible thoughts and actions which could be taken. The purpose of this essay is to suggest topics which can be used while you are in school, in a teaching position (in or out of school), or in the movement.

A good but difficult starting point for work on a variety of topics is the Pentagon Papers. Whereas their release three years ago created a great furor and aided in delegitimizing the government's case for waging war in Indochina, the mass of documents and analysis has often confused and bewildered the prospective reader. Dan Ellsberg has noted that his *act* has had more analysis and impact than the content. Movement people have been reluctant to read and consider the import of the Papers. Of course, not only is the mere reading a formidable task, but negotiating one's way among the contradictory documents and editions requires immense patience and free time. Three different and variant editions have been published (Beacon Press edition, *New York Times* edition published by Bantam, and the Defense Department edition from the U.S. Government Printing Office); all have discrepancies and deletions, all organize their content differently, and all range over different and overlapping time periods.

In addition to the confusion of the editions, the documents themselves were edited by a phalanx of government analysts under the direction of Leslie Gelb. The extent and length of documents often differ rather drastically from one edition to another. One must not only check one's documents but also distinguish between the documents and the analysis. Since the PP were prepared for the then-Secretary of Defense Robert MacNamara, the analysis was intended to provide a rationale for the war itself. Thus, when people use the PP, they can easily confuse the main actors – the authors of the original documents – with the analysts who tried to interpret them.

In an effort to resolve some of the problems of approaching the PP, Noam Chomsky and Howard Zinn have collected 15 essays from a variety of anti-war activists under the title: *The Pentagon Papers: Critical Essays.* The last hundred pages contain comparisons of the three known editions, a hitherto expurgated account of the Tonkin Gulf incident, indices and glossary for the four-volume Gravel/Beacon edition, a name index, a subject index and a general glossary. Because the authors wrote their critiques independently, they sometimes provide uncoordinated, repetitive and at times conflicting views. The authors do not adhere to any overall methodology or set of definitions. At times the lines between the analytical and documentary portions is blurred. However, if this volume is read as a handbook to specific issues in the PP, and as a guide to the various editions, then it is invaluable. In what follows, I will indicate the main thrust of each article.

Gabriel Kolko provides the framework for the essays by placing the war in the global context of American strategy after World War II. The development of the domino theory (as applied at first to Greece in 1947, next Korea, and then to Vietnam in 1952) was inextricably related to America's economic and military priorities. Even before the Korean war, this theory was magnified in importance by the supposed threat of grotesque, irrational, and monolithic communism. Should Indochina be lost, economic dislocation would force our major pacific ally, Japan, to eventual accomodation to communism. Even Europe would be threatened.

As Kolko has argued before, the Vietnamese resistance effectively prevented America from creating a global empire. The crisis of American power in Vietnam was not only caused by the political/military strength of the revolutionaries, but also by the limitations of America's presence in Vietnam. Continuous additions of U.S. troops threatened the fabric of Saigon's economy and society. However, instead of withdrawal, the Americans promoted a "punitive bombing campaign" designed to destroy the "will" of the regime. The explicit justification for this change in strategy gives weight to the charge of U.S. war crimes in Indochina.

Kolko's arguments for the existence of economic causes of the war are developed by Richard B. DuBoff in "Business Ideology and Foreign Policy." The thesis is that the National Security Council (NSC), established in 1947, "strengthened the bond between the formal policy-making apparatus culminating in the Presidency and the corporate-business-banking sector of the external society." (p. 20) With this in mind, the NSC documents in the PP constitute "paradigm statements" for our involvement in the war. DuBoff summarizes and comments on eleven of the most critical NSC papers from December 1949 to December 1954. The recurrent theme is America's economic and political commitment not to lose the area for fear of economic harm to Japan, and for fear of loss of credibility to America. One point is made exceptionally clear: The U.S. must not negotiate with the Communists.

Truong Buu Lam presents the only direct Vietnamese viewpoint. In his account of American aggression, he differs from the globalistic views of Kolko and DuBoff. Unlike Kolko, who seems to deny the importance of the civil war element and stresses only the international liberation aspects, Lam insists the war is complicated because America has aggravated a civil war within a revolutionary struggle. Use of this approach moves readers closer to an understanding of local politics and successful American manipulation of Vietnamese officials in order to serve its own interests. Lam effectively destroys any image of the "innocence" of American actions in Vietnam.

With the local regime totally dependent on U.S. aid and torn by internal feuds, the Americans could only trust their own people. Thus all counter-insurgency operations were increasingly Americanized. When this failed, the Americans singled out one group, totally isolated from any large social base, for support, and then provided it with excessive power to repress and oppress the rest of the society.

James Aronson provides a critical attack on the role of the media in handling the Vietnam war. He points out that they were too busy accepting the official sources to inquire about the real war. The only "valid" source was considered to be a government one. The "other" side was never "credible." The pro-Vietnamese journalist Wilfred Burchett, who had reported from Korea and from Vietnam, was not given equal time — indeed, he was totally ignored in the commercial press. The same build-up was given to all campaigns the U.S. Government wanted publicized. A few Young Turks exposed Diem but only in an attempt to change Washington's tactics, not to expose or oppose U.S. resolve to "save" South Vietnam at all costs.

Aronson discusses the publication of the PP and compares this with the *New York Times'* other refusal to publish news of the impending CIA invasion of Cuba. (Only recently have we learned that the *Times* killed a story on the invasion of Cambodia. Kissinger intervened to have the leak on the invasion quashed.) The PP were published, it was argued, because it would not harm the U.S.; the latter kept secret because they would harm U.S. security. What, Aronson asks, should be the standard for publishing? (Art Waskow has dealt in some detail with the politics behind the publication of the PP. He sees it not in terms of national security issues, but as a reflection of a split within the ruling class, their middle-level subordinates, and the media. See his insider's article in the first issue of *Peace and Change: A Journal of Peace Research.* Fall 1972. California State College, Sonoma, Calif.)

As if to get the point home, the next author is Wilfred Burchett presenting "the other side." His analysis of the beginning of the American presence in the Vietnam War, the Staley Plan, the Tonkin Gulf incident, etc. is based on personal observations, official documents, and, most revealingly and persuasively, on the first documentary and lengthly Hanoi reaction to the PP: *Les Vrais and les Faux Secrets du Pentagone* — True and False Secrets from the Pentagon. (It would be most helpful to have this book made widely available in the States.) The information in this book provides us with a detailed and different description of the Tonkin Gulf incident: the alleged second attack August 4th was a phantom of American policy makers, there were four (not one or two) U.S. destroyers, and the U.S. was definitely the provocateur. Other equally false American propaganda has also been unravelled by this work.

Gérard Chaliand's "Ideology and Society" and Don Luce's " 'Tell Our Friends that We're People' " stress the fundamental lacunae in the thinking of our political elites: their obdurate conceit is to belittle the values and goals of Vietnamese nationalists and anti-imperialists; to consider only the ideas of the U.S. Government and its actions "rational" and to reduce the Vietnamese peasant to an abstraction — a body count in a clicking military abacus. Our government ideology, never self-consciously expressed by the ethnocentric American advisors, has devastated both our enemy's country and our own.

John Dower's article on Japan merits considerable attention. Based in part on his Ph.D. thesis, it is the best researched analytical study of the American-sponsored Japanese stake and role in Southeast Asia. The military and economic U.S.-Japan "interlock" is elaborated in six readable and sometimes humorous sections. A mine of research topics is unearthed (see especially the list on top of page 107). A study of the political as well as economic effects of Japanese reparations would test the radical thesis ably developed by Dower and Chomsky that the U.S. is in Indochina to preserve markets and provide protection for Japan.

Nina Adams' "The Last Line of Defense" is an able sarcastic blast against the ignorance and errors of the American policy makers and analysts. The "expertise" and "noble" purpose which the writers assume for U.S. advisors and policies is in fact a shroud around the real crimes and hides the connections between "aggressive foreign policy and domestic repression." I must say "The Last Line of Defense" is an absolute must for introducing the mental set of the authors and analysts of the Pentagon Papers, is exceptionally well written with a bright and fast style, and supplies the reader an excellent annotated bibliography on Vietnam, Indochina, the war, and our involvement.

The next four articles discuss specific errors, misinterpretations, and falsifications. Philippe Devillers focuses on the 1950 decision to support the French in Indochina and the politics involved in our supporting the partition of Vietnam after 1954. Unique and tantalizing in his argument is his theory that the U.S. ruling class was split between the West Coast/South West *nouveau riche* military-industrial corporations (the "sun belt"), plus the China Lobby which wanted to interfere militarily in Indochina and supplant the French, and the East Coast establishment which wanted to aid and work through the French. (The same type of struggle occurred in the first decades of the century between investment — Morgan — and commercial — Rockefeller — banking interests. Then the question was whether or not to work through England's banking system or compete with it. See Carl Parrini, *Heir to Empire: United States Economic Diplomacy, 1916-1923.* University of Pittsburgh Press, 1969)

Chomsky's documentary analysis establishes the propagandistic effect of the analysis. Nowhere do the PP analysts basically deviate from the standard anticommunist political line of the government. Although the documents themselves are often revealing, the analysis is fraught with misinterpretations and contradictions. Chomsky dwells on the study's presentation of the formation of the NLF insurgency with particular reference to the fifteenth plenum of May 1959.

David Marr's autopsy of the counter-insurgency program provides a clear understanding of why the intelligence system was inaccurate and deceptive. As new policies based on incorrect data (and this data became the substance for the reports and evaluation in the PP) proved unworkable, the U.S. was forced to rely on more violence and terror. Or in the words of Robert Komer, "Few of our programs — civil or military — are very efficient, but we are grinding the enemy down by sheer weight and mass. . . ." The Pentagon study has the same effect. It "is much like the American operation it tries to describe — an enormous, overpowering, resourceful and misdirected effort which could crush by its sheer weight if it failed to convince by its arguments." (Adams, "The Last Line of Defense," p. 143)

The most meticulous and lengthy essay is produced by Peter Dale Scott on the National Security Action Memorandum (NSAM) #273 of November 26, 1963. This document has been cited to prove that LBJ was just following Kennedy's policies in Vietnam. In fact, it specifically reversed our strategy from a cautious withdrawal to an open military commitment. In the appendices, Scott has laboriously

reconstructed parts of the text from several sources and compared it to other documents. His research proves how carefully one must use the materials, not to speak of the analysis, in the PP.

Walt Haney does not limit himself to his title, "The Pentagon Papers and the United States Involvement in Laos" but provides an extensive history of the war from 1950 to 1971. His essay is valuable for its discussion of both U.S. and DRV interests in and policies toward Laos. The propaganda about the DRV's attack on December 31, 1963, is shown to be pure fabrication. The domestic policies and politicians are well described. Haney makes clear that the "secret" war (or in the ethnocentric and racist words of Dean Rusk, the "wart on the hog of Vietnam") was a secret and a minor irritation only to the American people. To the Laotians it was two decades of open terror. When Johnson publicly stopped bombing North Vietnam, he secretly increased bombing missions over Laos. When the cease-fire was signed in Paris, bombing increased on Laos and Cambodia. Yet Americans were told that Johnson and Nixon brought peace.

All of the writers have had one major purpose – to expose the real causes and goals of American policies in Vietnam and to point out the PP's inadequacy in this task. Although the last author, Fred Branfman, does describe in detail the development and effects of the bombing war (what he calls the "Third Indochina War" since 1968), he is disturbed by the technocratic mentality of the writers and analysts of the PP. Branfman is concerned about the morality of the architects of the war. This is indeed the age, prophesied by Anatole France in *Penguin Island,* when a man from a space ship would push a button, destroy a country and only be emotional about the fear that he would be late for dinner. Or as expressed by the American beat poet and satirist Kenneth Patchen: In order to apply for a job they told him he had to dig up a relative from the grave, destroy a small village, destroy a major country and finally destroy the world. He complied grudgingly but efficiently. When asked to put it all back together, "Well . . . it was my turn then to tell *them* something! Shucks, I didn't want any job that bad." ("In Order To," *Poems of Humor and Protest,* City Lights Books.)

Branfman begins his searching, almost religious essay with a quotation from the London *Observer* of 1944 which claimed that we may get rid of the Hitlers, but the Speers "will long be with us." This was to be their age. Ironically, it was Speer's later confession which catalyzed Ellsberg into acting with even greater conviction.

Richard Kagan

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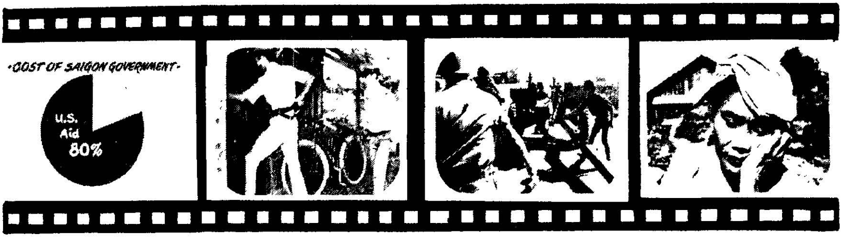

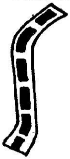

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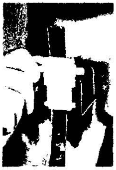

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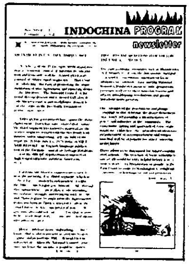

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